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VOLUME 116
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JUNE 1960

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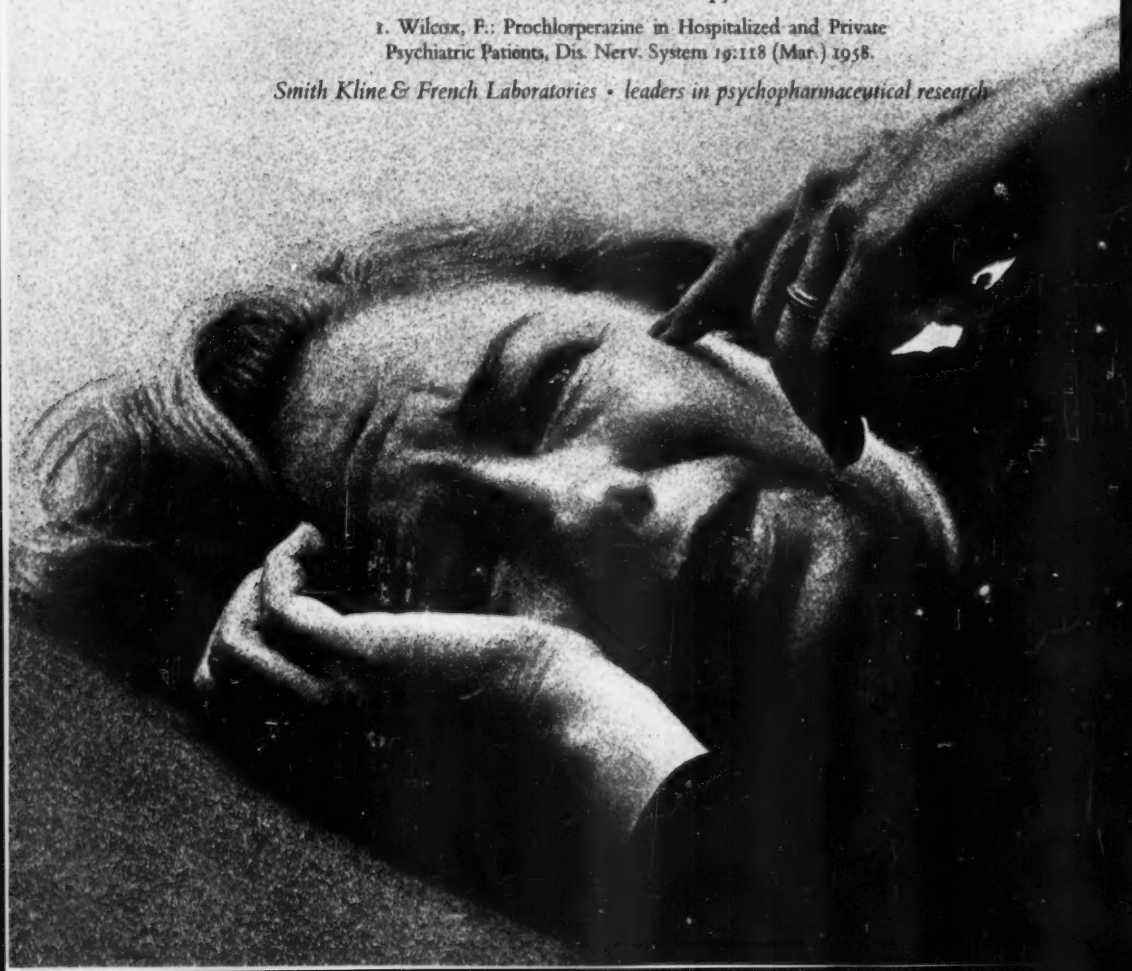

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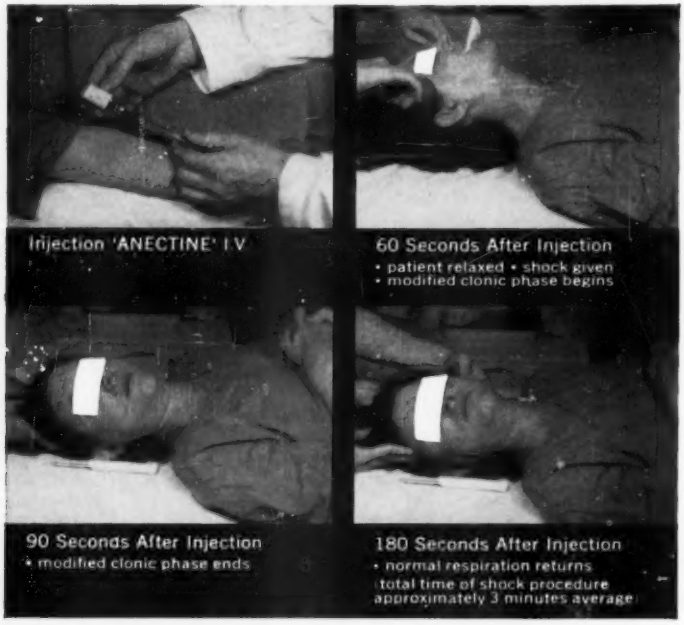

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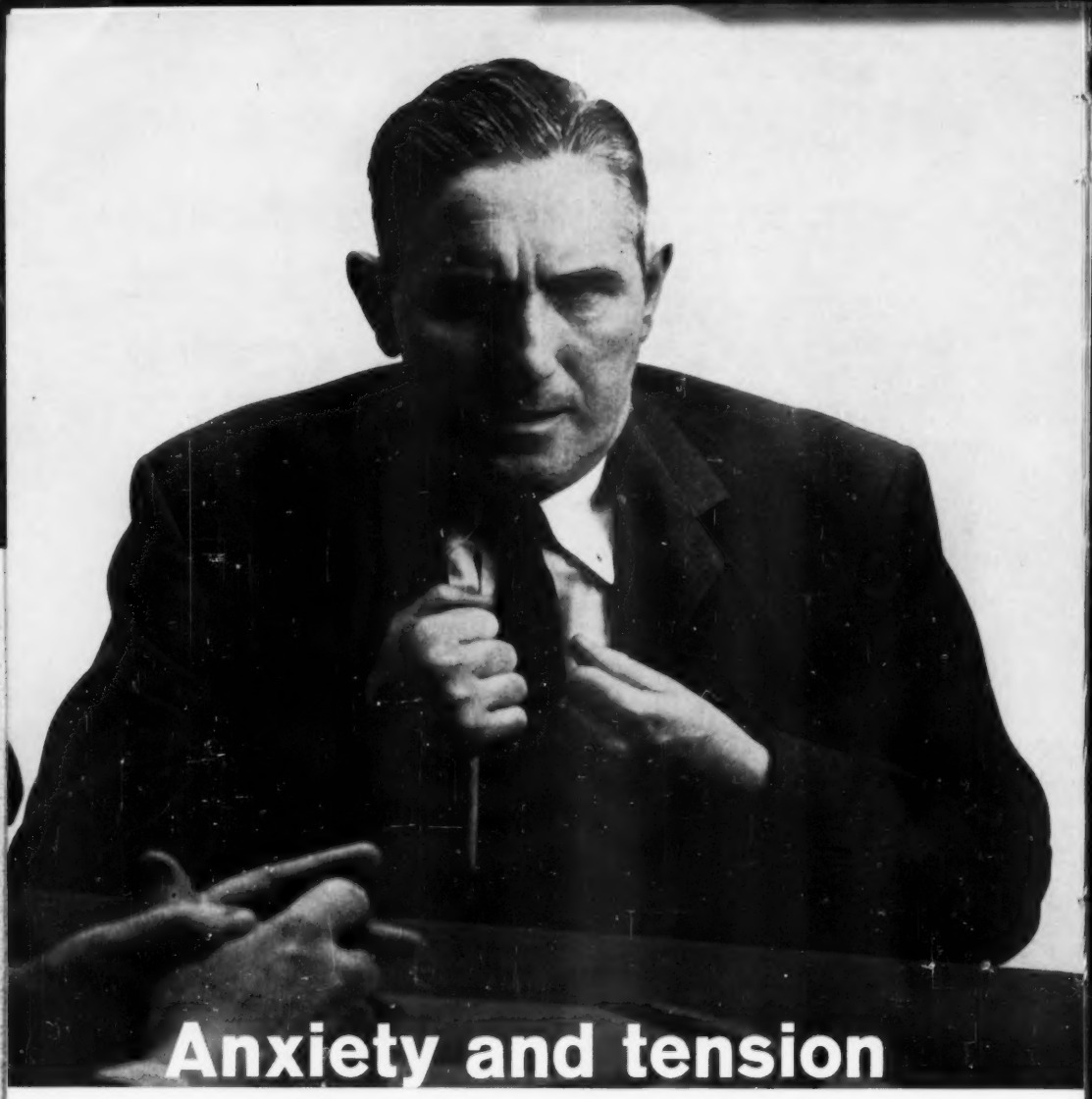

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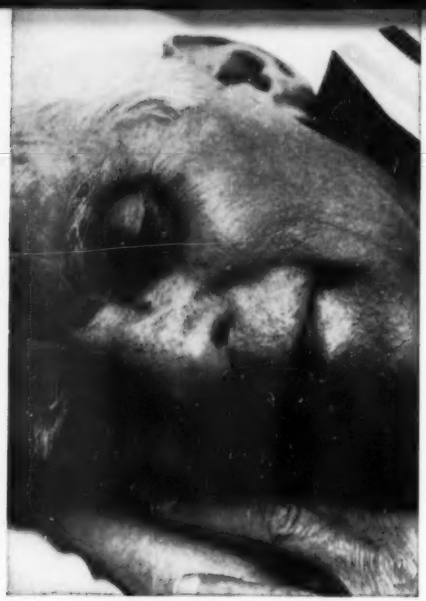

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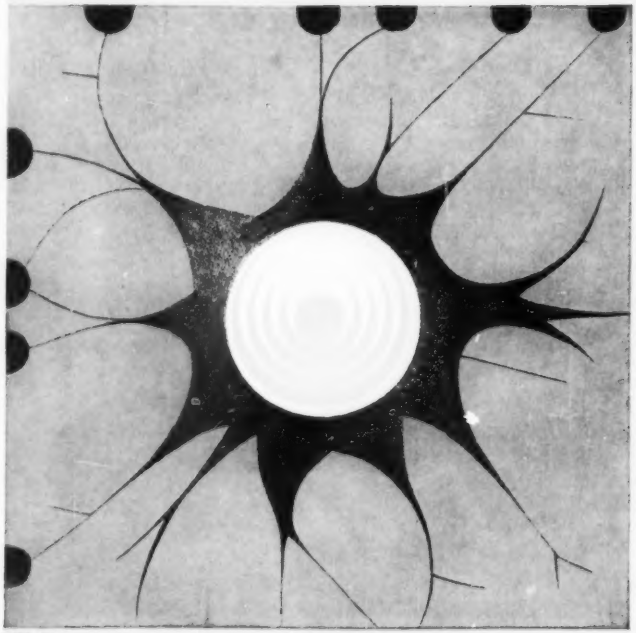

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THE MODERN TREATMENT OF DEPRESSIVE DISORDERS [1, 2]

FRITZ A. FREYHAN, M.D.[3]

The era of the modern treatment of depressive disorders started with Cerletti's and Bini's demonstration of the value of electroconvulsive therapy. This method was widely, though not universally, accepted as constituting the most effective, safe and rapid treatment for the various depressive psychoses. With the establishment of an effective therapeutic procedure, psychiatric interest in the affective disorders subsided. Whereas much therapeutic and investigative effort was centered on problems of schizophrenia and the psychoneuroses, the knowledge on questions of epidemiological, pathogenetic and therapeutic aspects of depressive disorders advanced very little. What compels psychiatrists today to recognize depression as one of the outstanding problems of psychopathology and therapy, is the result of recent developments in psychopharmacology. The search for compounds with potent antidepressive properties has not only produced new drugs but new concepts on etiology. While the research initiative came from the laboratories, the clinicians are once more confronted with many controversial questions on all fronts which had remained unresolved in the past. The McGill University Conference on Depression which was held in Montreal last March(1), demonstrated not only how wide the diversity of psychopathological concepts has remained, but revealed at the same time the astonishing gap between clinical impressions, theoretical assumptions and established facts. The lack of knowledge and agreement on questions of diagnostic differentiation, the limited information on incidence and long-term prognosis and, finally, the conspicuous absence of valid evaluations of the effectiveness of ECT, of psychotherapy and of various modes of therapeutic management must be remembered if new approaches, whether theoretical or therapeutic, are to be clinically meaningful and scientifically productive.

Perhaps in no other branch of medicine can one find such an extraordinary variance of results with the same therapeutic methods as in psychiatry. Psychiatrists do not commonly share comparable therapeutic experiences since they deal with different patient populations, guided by contrasting conceptual frameworks. The controversial literature on electroconvulsive treatment can serve as one of many examples. ECT has been praised or condemned on clinical, theoretical and aesthetical grounds. Its undeniable benefits have been interpreted from somatic and psychodynamic points of view. The magnitude of its impact on recovery is still a matter of dispute. Kalinowsky writes : "The response of depressions to usually less than 5 convulsions, pharmacologically or electrically induced, is one of the most predictable events in psychiatric treatment"(2). But Manfred Bleuler (3), Kielholz(4), Lewrenz(5) and other continental psychiatrists have always maintained that the ECT-induced disappearance of depressive symptoms is not indicative of an actual change in the spontaneous course of the depression. Although their concept of endogenously determined depressive phases is of great significance for the evaluation of therapeutic procedures, there is little agreement on what constitutes the spontaneous course of a depressive psychosis. It is generally believed, and has been supported by statistics, that ECT has greatly accelerated recovery from depressions. This is reflected in earlier discharges from psychiatric hospitals and institutions.

But the use of hospital statistics for the purpose of demonstrating therapeutic achievements is a most difficult and ambiguous endeavor. To illustrate this, I

[1] Read at Philadelphia Psychiatric Society Meeting, November 13th, 1959.

[2] This study was supported in part by a grant (MY-2991) from the National Institute of Mental Health, U. S. Public Health Service.

[3] Adj. Associate Professor, University of Pennsylvania and Clinical Director, Director of Research, Delaware State Hospital, Farnhurst, Del.

should like to examine data from the records of the Delaware State Hospital. It is the particular advantage of this hospital that it is the only psychiatric hospital for the population of the entire state. Since depressive disorders tend to be periodical and since relapses mean readmission to the same hospital, the hospital's data may well be regarded as being indicative of certain changes during the first 5 decades of this century. The figures represent the total number of female patients with depressive disorders of the manic-depressive variety who were admitted to the Delaware State Hospital in the years 1900–1957. Table 1 shows a division into 3 series: 1. Patients admitted before the introduction of ECT in 1938; 2. Patients admitted since 1938 who received ECT; and 3. Patients admitted since 1938 who did not receive ECT. The figures in each series show the average duration of the first period of hospitalization and the average length of the interval between first and second admission for those patients who relapsed.

What information can this survey supply? It is evident that the patients admitted before 1938 spent the longest time in the hospital. After 1938, there is a change: the duration of hospitalization decreases for both patients who did and who did not receive ECT. But if only two series had been shown, namely the series before 1938 and the series of ECT-treated cases since then, one may have been tempted to attribute the change solely to ECT.

Equally intriguing are the figures for the length of the interval between first and second hospitalization. Perhaps contrary to expectation, there is a suggestion of shorter intervals since 1938. This would seem to confirm previous reports in the literature which claimed earlier relapses after ECT. Some interpreted this as an indication of some damaging effect. But this seems unlikely since the decrease of the interval

TABLE 1

DURATION OF PERIOD OF FIRST HOSPITALIZATION AND SEPARATION: CYCLOTHYMIC DEPRESSIONS, FEMALES, 1900-1957

DURATION OF FIRST HOSPITALIZATION	1900-1937		1938-1957 ECT		1938-1957 Without ECT	
Age on Admission	Number Patients	Days	Number Patients	Days	Number Patients	Days
-30	27	204	36	96	33	101
30-49	57	334	78	203	66	84
50-69	28	294	37	184	40	97
70+	1	244	1	159	3	133
Totals	113	292	152	173	142	93
DURATION OF FIRST SEPARATION						
-30	18	3054	12	661	18	1874
30-49	24	1987	29	1676	24	1472
50-69	11	1880	16	1425	17	1462
70+					1	246
Totals	53	2327	57	1392	60	1569

appears to be shared by patients with and without ECT.

Perhaps these figures have social implications as well. The long period of hospitalization before 1938 may reflect the conservative, if not anxious, attitude towards psychiatric patients which prevailed at that time. And the longer interval for the first series may not indicate better therapeutic results, but greater hesitation on the part of the families to return the patient to the hospital, whereas the more recent acceptance of psychiatric treatment favored earlier readmissions.

If, for the purpose of therapeutic assessments, patients are used as their own controls, accurate information is needed on all aspects of past illness and treatment. To illustrate the extent to which variation and variability manifest themselves in the course of periodical depressions, I have selected from our Tofrānil series those 16 patients who have in common a minimum of 4 depressive phases requiring hospitalization. Table 2 lists, in terms of months, duration of hospitalization (H) and interval (I) for each depression. The table shows furthermore what somatic treatment, if any, the patient received during each period of hospitalization. A case by case examination reveals little evidence of a consistent interdependence between therapeutic methods, length of hospitalization or length of interval. What this illustrates is the fact that variation and variability of clinical aspects of the depressive psychoses in the same person as well as from person to person render difficult the establishment of homogenous groups for comparative purposes. This lesson has yet to be learned by clinicians, research psychologists and biostatisticians who direct psychopharmacological investigations.

It is one of the most important contributions of clinical psychopharmacology that attention has been drawn to the relationship between therapeutic modes of action and specific psychopathological symptoms. For the understanding of the effects of psychotropic drugs, it is by far more informative to relate these effects to particular psychopathological states than to diagnostic en-

TABLE 2

CATAMNESTIC PROFILE OF 16 CYCLOTHYMIC PATIENTS WITH 4 OR MORE ADMISSIONS

No.	Sex	Adm. Age	I H	I I	II H	II I	III H	III I	IV H	IV I	V H	V I	VI H	VI I	VII H	VII I	VIII H	VIII I	IX H	IX I	X H	X I
1	F	25	3	222	1	27	1	15	1	4	2	84	2	10	2	6	2	12	2	18	2	2
2	F	37	6	2	9	192	3	17	2	1	8	1	8	2	3	7						
3	F	51	3	22	2	10	3	8	2	31	3	18	2	1	3	4						
4	F	37	1	1	9	120	2	96	5	32	9	18	2	1								
5	M	38	22	147	2	68	2	1	110	2	4	3	11									
6	F	52	2	60	5	36	1	12	1	60	1	1	3									
7	M	33	4	48	21	39	4	38	5	61	18	9										
8	M	34	1	208	1	91	1	23	2	3	1	12										
9	F	55	3	2	4	8	5	8	3	11	4	11										
10	F	67	1	15	1	46	11	2	1	8	4	9										
11	M	58	1	8	1	24	5	70	3	2	7											
12	F	55	7	102	3	65	3	33	5	7												
13	F	38	1	36	4	12	4	94	3	8												
14	M	50	4	66	5	21	2	49	3	1												
15	F	49	2	3	3	12	L 1	84	29	5												
16	F	56	2	42	5	20	3	91	1													

Without ECT
ECT
TOFRANIL
Combined
L Lobotomy

Status as of March 1, 1959.

tities. A clinical study of drug effects depends on the proper identification of "target symptoms" on which the drug can exert its action. Amphetamines, for example, are commonly prescribed to reduce appetite. The greater majority of patients who take amphetamines for this purpose, do not experience any psychotropic effects. But if prescribed as anorexant for patients with unrecognized symptoms of mild depression, they report, often enthusiastically, a mood-lifting effect as well as an increase in initiative. The same drug, then, exerts its psychopharmacological action primarily in those persons who present in fact a psychopathological target for psychotropic effects.

The present era of antidepressant pharmacotherapy started with the introduction of iproniazid and the concept of "psychic energizers." It is not yet known whether monoamine oxydase inhibition corrects a biochemical deficiency which is assumed to be the cause of depressions. There is no agreement at this time whether the monoamine oxydase inhibiting drugs achieve their antidepressant effects on the basis of MAO-inhibition. Nor has it been proved which biochemical deficiency plays a decisive role in the pathogenesis of depressions in the first place. States of mental depression differ widely in symptomatology, incidence and prognosis. From the clinical point of view, one must, therefore, caution against unitary concepts which concentrate attention on therapeutic agents with one-directional modes of action. It seems unreasonable to expect that the same drug calms the anxious, stimulates the apathetic and inhibits the agitated or self-destructive patient. Pharmacotherapy with antidepressant compounds is still in the exploratory stage. It would be premature to confine clinical studies to particular types of compounds.

OBSERVATIONS WITH ANTIDEPRESSANT DRUGS

The clinical investigations of 5 antidepressant compounds [4] were carried out by the Delaware State Hospital's research department. Only hospitalized patients are included in this study. The selection of patients for treatment with antidepressant drugs was based on the diagnosis of a depressive disorder as well as on specified psychopathological symptoms. We distinguish between behavioral, somatic and experiential symptoms. "Behavioral" symptoms refer to observable phenomena such as withdrawal, listlessness, sadness, agitation, self-mutilation, lack of initiative, *etc.* "Somatic" symptoms include disturbances of appetite, sleep and elimination as well as the various functional disturbances which are part and parcel of depressive disorders. The subjective account of the depression, *i.e.* anhedonia, lack of vitality, guilt feelings, ideas of hopelessness, hallucinations and delusions and so on are grouped as "experiential" symptoms. Evaluations are based on changes of the individual psychopathological profile. Daily observational protocols were kept separately by psychiatrists and nurses. All clinical observations whether by nurse or psychiatrist were unsolicited. While rating scales direct the observer's attention to predetermined items, it is the purpose of our protocols to obtain spontaneous observation on the widest possible scale. It is the clinical investigator's task to discover drug effects, to identify variables of action and interaction and to explore the range of clinical effectiveness on the basis of clinical knowledge and experience. The final evaluations reported as "results" reflect a patient's status at termination of treatment regardless of clinical or administrative consequences in terms of further hospitalization or discharge. Therapeutic results are divided into 3 groups: (a) optimal, *i.e.* total disappearance of the target symptoms; (b) partial modification; and (c) failure. As a matter of principle, drugs which are available in ampules are given by injection for about 1 week before oral medication is substituted. One cannot sufficiently emphasize the need for the closest possible supervision of the administration of drugs. Psychiatric patients in general and depressed patients in particular are apt to deceive physician and nurse. The actual administration of drugs is not infrequently the weakest link in otherwise

[4] We wish to thank Geigy for their generous supplies of Tofranil; Smith, Kline & French Laboratories for SKF 385 and SKF 6270; Warner-Chilcott Laboratories for Nardil and Pfizer Laboratories for Niamid.

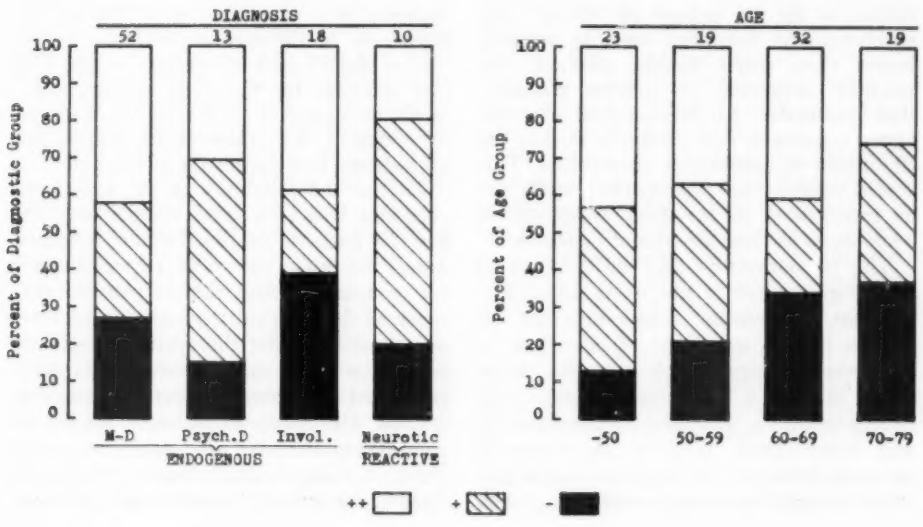
highly sophisticated experimental investigations. No other drugs should be permitted during the therapeutic course as it is obviously impossible to explore the effects of a compound without restricting medication to the drug under study.

The 5 compounds included in this report fall into 3 pharmacological groups : 1. *The monoamine oxydase inhibitors* represented by nialamide, phenelzine, and a trial preparation SKF 385 (trans-dl-2-phenylcyclopropylamine hydrochloride) which, though structurally related to the amphetamines, is a far more potent inhibitor of monoamine oxydase than iproniazid ; 2. Trial compound SKF 6270, a *phenothiazine* (10-(3-dimethylamino-2-methylpropyl)-2-methylthiphenothiazine hydrochloride), which is less potent than chlorpromazine to which it is structurally related ; and 3. Imipramine which is different from all other antidepressant compounds since it does not have a stimulating or energizing action and is not a MAO-inhibitor. The pharmacological uniqueness of imipramine is of special significance since on one hand its action cannot be explained in terms of enzyme inhibition, while on the other hand it appears to be the most effective antidepressant drug which is now available.

Our investigations of the antidepressant properties of imipramine, now known as Tofrānil, commenced nearly 2 years ago. The results of this investigation have been reported previously(6), and are briefly summarized here. The antidepressive action of imipramine is selective. While the range of this selective action does not generally coincide with diagnostic groups, the most favorable results were obtained in the treatment of patients with cyclothymic psychoses. Therapeutic results shown in Fig. 1 are similar to those reported in Switzerland, France, Germany and Canada where clinical studies have been in progress for 3 years. The relatively low rate of total failures justifies the position that intensive imipramine treatment should be tried routinely before ECT is instituted. Patients under 60 years had significantly better results than those above the age of 60. Since both age groups consist for the greater part of manic-depressive patients, the age factor would seem to have an influence on therapeutic outcome. We found that the majority of patients who eventually respond with

FIGURE 1

TOFRĀNIL : RESULTS BY DIAGNOSIS AND AGE



optimal results, show the first favorable change within 3 to 6 days on 150 mg. a day. An early modification of depressive symptoms is crucial for the evaluation of antidepressant action. If one has to wait 2, 3 or more weeks before favorable changes occur, one can never be certain whether to attribute these changes to treatment or to spontaneous recovery. Moreover, if pharmacological treatment is to replace ECT, its effects must be rapid.

What we observe in the initial phases of imipramine treatment is mostly a decrease in the intensity of depressive symptoms. The patient experiences a return to normality but does not manifest a drug-induced psychoaffective syndrome such as euphoria or overactivity. There are various somatic manifestations which reflect the drug's central mode of action. Dizziness and feelings of weakness occur during the earliest phases of treatment. Mild tremors and feelings of shakiness may last somewhat longer as do perspiration, paresthesias and constipation. While none of these somatic effects is severe, they are more disturbing to patients who fail to come out of the depressive state. Although imipramine does not exert a stimulating effect, a switch from depression to hypomanic and excited states has been observed in some patients with cyclothymic psychoses. Inasfar as differential effects on particular types of target symptoms are concerned, disturbances in the sphere of vitality and psychosomatic functions tend to respond better than panic moods, phobias and nihilistic delusions. It appears plausible that particular psychobiological disturbances constitute more suitable targets for the action of imipramine than others. This would explain why therapeutic results are as pronounced in reversing symptoms in some cases as they are absent in others.

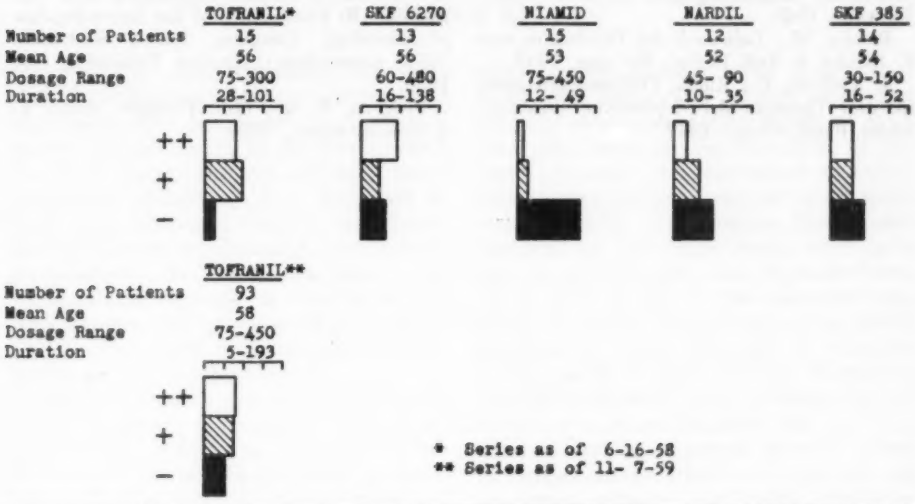
The phenothiazine SKF 6270 is not in the proper sense of the word an antidepressant. Neuroleptic compounds are effective in the treatment of disorders of whatever etiology which manifest themselves through a behavioral common path of hypermotility, increased affective tension and hypernormal initiative. By virtue of their inhibitory action, they are useless and often harmful for patients with energy deficits and affective retardation. Several French workers reported, however, therapeutic success with levomepromazine which is structurally quite similar to SKF 6270. Levomepromazine, made available to me as SKF 5116, and SKF 6270 facilitate sleep very effectively. SKF 6270 decreases abnormal initiative but does not produce extra-pyramidal symptoms of significance at therapeutic dose levels. Drowsiness, hyposalivation and constipation are the most frequently encountered somatic manifestations. Our observations revealed that patients with panic, agitation and severe insomnia responded quite favorably. This would seem to suggest that this compound qualifies for the treatment of specific depressive syndromes.

Inasfar as the MAO-inhibiting drugs are concerned, our investigations have not progressed far enough to reach definite conclusions. But neither iproniazid nor the newer compounds have yet convinced us of their therapeutic effectiveness in the treatment of depressive psychoses. Generally speaking, our results are less promising than one would be led to expect from current publications. Shown on Fig. 2 is a survey of findings for each of the 5 compounds. In all, this study includes 147 patients, 111 women and 36 men. This ratio of 3:1 corresponds to the sex distribution of admissions, confirming reports in the literature that depression is far more common in women than men. The highest frequency of depressive psychoses occurs during the 5th and 6th decade in life. This fact accounts for the close approximation of the mean age of the 6 samples. With the exception of 16 psychoneurotic depressions, all patients had depressive psychoses.

To test the reliability of the initial impressions based on small sample size, the first 15 patients in the Tofrānil (imipramine) series are shown as a special group for comparison with the 93 patients who received the drug subsequently. It has been our experience that therapeutic trends observed on small samples are substantially confirmed by subsequent extensive investigations. This seems to be borne out by the comparison between the small and large Tofrānil (imipramine) series. While the data based on small samples are of neces-

FIGURE 2

CLINICAL EFFECTIVENESS OF ANTIDEPRESSANT DRUGS



sity tentative and reflect at best therapeutic trends, there would seem to be significance in a comparison of failure rates. Clinical evaluations may easily differ in matters of degree of improvement; they rarely disagree on failures. Among the MAO-inhibitors, Niamid (nialamide) seems practically ineffective. Nardil (phenelzine) and SKF 385 manifest therapeutic activity on a more substantial scale although both have failure rates of 50% and 42.8%, respectively. While more extensive studies may reveal a broader therapeutic range, it is only too obvious that these results do not justify the extraordinary promotional claims which are directed not only at psychiatrists but the general practitioners as well. There is real danger that competent psychiatric treatment of depressed patients may be reduced to simple prescription writing, and prescription of some ineffective drugs at that.

SUMMARY AND CONCLUSION

The pharmacological treatment of depressions offers this immense psychological advantage: the patient maintains his experiential continuity. The amnestic syndrome associated with ECT, to which many attributed therapeutic significance, proves to be quite superfluous as is seen in successful pharmacotherapy. The preservation of experiential continuity has vast implications for psychotherapy. Until now, psychotherapy either followed ECT or had to be limited to patients who seemed capable of affective contact and of self-control over suicidal impulses. With ECT, the patient remains physically and emotionally passive. His recovery comes, as it were, from without. Pharmacotherapy makes him a participating partner. This offers psychotherapy entirely new opportunities to involve the patient in the therapeutic process until recovery is seen as coming from within.

The treatment of depression is once again in transition. If psychiatric history is not to repeat itself, we must realize that the measure of success will depend on our capacity to abandon static positions, to integrate new knowledge and to create comprehensive therapeutic methods.

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SOCIAL CLASS AND MENTAL ILLNESS: SOME IMPLICATIONS FOR CLINICAL THEORY AND PRACTICE

RAYMOND G. HUNT [1]

One of the issues motivating frequently voiced disaffections between clinicians and experimentalists has been the purported heavy reliance placed by the former upon "intuitive" operations and clinically based theoretical excursions. Not the least of these have related to the clinician's heavy indebtedness to psychoanalytic and related formulations. Numerous criticisms (*e.g.*, 9) of clinical concepts and methodologies have centered about these issues, and especially in respect to personality, psychopathology and psychotherapy.

While the present paper certainly has no desire to gainsay the value of clinical evidence and techniques, as such, it is intended to point out certain pertinent questions about them which arise in the particular light of recent empirical investigations of relationships between psychopathological conditions and their treatment, on the one hand, and socio-cultural factors on the other.

The comments which follow will certainly not be unfamiliar to most. They may even appear banal. The point is that they may no longer be ignored as they have commonly been in the past.

SOCIAL CLASS AND MENTAL ILLNESS

Most notable among the studies alluded to have been those conducted in the New Haven, Conn. area by the research team headed by A. B. Hollingshead and F. C. Redlich(2). In this unusually well done research the investigators discovered marked social-class-linked variations in both the prevalence and incidence of mental illness and also in their treatment.

After classifying the patients in their psychiatric population into one of 5 social class levels defined according to the patient's occupation and education, Hollingshead and Redlich analyzed the kinds of treatment received by these patients for their illnesses as a function of their social class position. Among other things, they found that lower class patients were most likely to receive no specific treatment at all (custodial care) or else some form of "somatic" therapy (*e.g.* convulsive therapy). Lower class patients were not at all likely to be receiving psychotherapy. Higher class patients, on the other hand, were most likely receiving some form of psychotherapy, at least as part of this treatment program. In further elucidation of the trends apparent in these data, the investigators proceeded to analyze the type of psychotherapy received by those patients who did receive it as related to social class position. They differentiated several general varities of psychotherapy which were, in order of presumed intensity and merit: psychoanalysis, analytic psychotherapy, eclectic psychotherapy, relationship therapy, group therapy. Again marked relationships were found. For example, if psychotherapy was received at all, the lower class patient was most likely to receive group therapy and never the most highly regarded type of psychotherapy, full psychonanalysis. The latter was virtually confined to the top status levels.

Thorne(14) has pointed out some serious flaws in this part of the Hollingshead-Redlich work. However, what is of primary importance here is the fact that lower class patients tend not to be found in the more personal, intimate and intensive forms of psychotherapy with the same frequency as are higher class patients.

It was also found that the social class position of the patient was closely correlated with the prestige of the professional personnel who played the major role in his treatment. High status patients were quite unlikely to be treated by low status (professional) personnel and low status patients were at least equally unlikely to be treated by high status practitioners. Finally, both the duration (as measured by total number of interviews) and the intensity (as measured by frequency of interviews)

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of psychotherapy were found to increase as a function of increasing social status.

A simple economic interpretation of these results, it is important to note, is not entirely tenable. While financial considerations very probably play a part, Hollingshead and Redlich have shown them not to be the sole factors (and perhaps not even the most important ones) responsible for the phenomena observed. Also significant is the fact that the patterns illuminated by Hollingshead and Redlich are not peculiar to the circumscribed area in which they worked. Other investigators in widely scattered communities have, on a smaller scale, reported findings consistent with those discussed in the preceding paragraphs (see Hunt(5), and Scott(13) for reviews of this literature). It seems quite apparent that the New Haven studies especially have pointed up an issue of significance, both to society and to the professions concerned. Two aspects of this issue are the concern of the present essay; one of these relates to the validity of clinical theory and the other to psychotherapy in particular.

SOCIAL CLASS AND CLINICAL SAMPLES

It is well known and surely axiomatic that generalization from any research must proceed with due respect to the nature of the subjects constituting the sample upon which that research was performed. Strictly speaking, generalizations should be restricted to populations of which the experimental sample is representative. Indeed researchers rarely follow this stricture to the letter, often speculatively referring their finding to much wider populations. Unquestionably this is both a defensible and desirable procedure. However, some explicit checking of such generalization should occur at strategic points and the need for checking should be seriously recognized if a discipline is to attain full scientific stature and to warrant the studied attention from others which it seeks.

Now there has often been a strong tendency on the part of clinical workers to apply in literal fashion, concepts and ideas essentially clinical in nature and origin to problems of "normal" behavior. This tendency has been clearest in respect to psychoanalysis, and especially in the case of Freud. Others beside the writer have pointed out that such applications run a serious risk of being unsound owing to the markedly non-representative character of the sample from which the generalized ideas are derived.

Data such as those from Yale, strongly suggest that the same cautions may be voiced in connection with theories of psychopathology, of the neuroses or even of a particular neurosis. They suggest that not only may samples be chronically biased with respect to the clinical-nonclinical dimension, they may be at least as seriously biased within the former. For instance, if psychoanalysts are treating primarily patients coming from the highest (status) 10 or 15% of the general population (and we now have evidence that, in general, this is the case) they are dealing with subject samples which are not only dubiously representative of the general population, but samples which are perhaps not even representative of clinical populations. Hence, to the extent clinical theory relies upon psychoanalytic and similar formulations derived from such samples (and it is quite apparent that it does, in large degree) the implications for its theorizing should be obvious.

We might point out that the natural tendency to view "cross-cultural" in terms solely of variations across widely divergent social groups (*e.g.*, Samoans *vs.* Western Europeans) represents too narrow an interpretation of the term. A truly cross-cultural regard (and it is this which is suggested as one deficiency in modern clinical theory) implies attention to variations across differentiable sub-cultural units *within* a larger social group. And, of course, a particularly important sub-cultural structure is that around which the present comments center, namely that of social class groupings within a given society.

No implication should be read into the foregoing remarks that clinicians should shut up shop as theoreticians and/or contributors to the growing fund of knowledge about behavior. What is suggested is a greater measure of caution in generalizing from potentially seriously biased "research" samples, a tighter rein on the theoretical impulse and most important, redoubled efforts

to operationalize psychoanalytic and other clinical concepts so as to make them more amenable to empirical-experimental study by more "tough-minded" colleagues.

SOCIAL CLASS AND PSYCHOTHERAPY

In respect to psychotherapy, data such as Hollingshead and Redlich's contain perhaps even more far reaching, though less direct, implications which become clear if we assume along with Ruesch and Bateson(12) that the essence of any psychotherapeutic process is communication. The heart of the process rests in the relationship between patient and therapist and their interaction one with the other. This interaction is a communication process as Ruesch and Bateson have cogently shown. Each party is, at one time or another, both a sender and a receiver of "messages," whether these be verbal or non-verbal. Each is continuously "encoding" and transmitting messages to the other and receiving and "decoding" others.

The success and efficiency of such a communicative process depends upon several factors, among the most basic of which is the necessity that each party share with the other a common "code." This in turn implies that they adhere to congruent linguistic conventions and that the conceptual and valuational orientations signaled by particular linguistic elements be shared, at least in general. Rapoport(10), moreover, has persuasively argued, "the impossibility of communication between two people who have not shared a common *experience*."[2] Should these conditions be lacking or rudimentary, the communication process will entail large amounts of "noise" and may break down entirely. At best, communication will be inefficient, suffering from severe distortions of various kinds and the consumption of large amounts of energy. At worst communication, as such, will cease, although it is possible that neither party will be fully aware of the immediate communicative state. If the latter be the case each party will function in an "as if" manner only—no real communication will occur.

That there will always be a fair amount of "semantic variation" (noise) in any communicative relationship of the scope of psychotherapeutic intercourse should be obvious. Indeed a good measure of the "work" in psychotherapy is directed toward reduction of these variations. Both patient and therapist try to "understand" the other. In order for this activity to be successful, however, these semantic problems must not be too pervasive or fundamental. Otherwise the "semantic barrier" will be insurmountable or will require inordinate expenditures of time and energy. Therefore it is suggested that successful psychotherapy (assuming it can be successful) requires as a prerequisite a linguistic and semantic framework the basic components of which are shared by patient and therapist. The therapist has neither the time nor the energy to adequately train the patient in the therapist's "code" when there are deep-rooted and widespread discrepancies. By the same token, the patient rarely has the patience to train the therapist and it must be remembered that the superficial fact that both therapist and patient speak "English" is no assurance of successful communication on anything but an equally superficial level, even though they may, at times, delude themselves to the contrary.

The possible relevance of these considerations is mentioned by Hollingshead and Redlich in their discussion of possible reasons for the dearth of psychotherapeutic endeavors involving lower class patients. In the light of the ideas just discussed the argument would run as follows: therapists will, by the nature of things, select patients with whom they feel they can work most effectively, *i.e.*, (while not necessarily a conscious criterion) patients with whom communication is both effective and efficient. In this manner the therapist comes to deal with patients with whom he shares congruent linguistic conventions and similar conceptual and valuational orientations. In other words, the therapist selects patients who, (a) "talk his language" and (b) express value orientations which are not alien to his own, thus leading him to the implicit belief that both will be able to "emphasize" with the other.[3]

Now there is a greater likelihood of the

[2] Italics in original.

[3] It is quite likely, of course, that the patient engages in a similar selection process, looking for a therapist in whom he can "feel confident."

therapist finding patients who satisfy these implicit criteria among people of backgrounds similar to his own. And among the pre-potent background factors which may be implicated is that of social class position. Since it can be shown that the vast majority of psychotherapists derive from middle class backgrounds(2, 4, 11), and since it can also be shown that the middle class is characterized by a particular set of linguistic conventions and value orientations (1, 3, 7, 8) which are at least partly different from those of other class groups, it is hardly surprising that the middle class therapist should fail to number quantities of lower class patients in his caseload, and this will be true whether he is in private practice or not. These may not in reality, be the people who will make the "best" patients for him. Because of this it is even possible that therapy will be less likely to succeed with lower class patients because of the high "noise" levels introduced into the communication process which is therapy. It might even be argued that the therapist *should* confine himself to middle class patients as it is with them that he operates most efficiently and so can do the greatest good per unit of time (*cf.*, Gurrslin, Hunt and Roach(3) for a discussion of this point).

PSYCHOTHERAPY AND THE MIDDLE CLASS ETHIC

There is another related idea which can be advanced in support of the thesis just developed. It was suggested by the writer in a recent review(6) of the Hollingshead-Redlich work that not only may the *practice* of psychotherapy be class-linked as they have shown, but perhaps the very *principles* of psychotherapy may be so linked. Since these have been developed mainly by middle-class practitioners in the course of their experiences mainly with middle class patients, it seems a good possibility that the therapeutic principles which have been generalized from these experiences may be intimately tied to the factors typifying these particular interactions.

A similar contention was developed some time ago by Kingsley Davis(1) in connection with the principles of mental hygiene. It was Davis' thesis that these "principles" were, in fact, little more than secular statements of the prevailing middle-class morality (the Protestant ethic) disguised under a superficial mantle of technical jargon and pseudo-scientific respectability. What is more, Davis was most pessimistic about the prospects for mental hygiene principles representing anything more than this. In any event, it does seem wise to entertain the possibility that what Davis contends to be true of general mental hygiene principles is no less true of psychotherapeutic principles. To the extent this is true, it is likely that not only will psychotherapeutic treatment of lower class patients be difficult and inefficient, it may actually not be practicable in any real sense.

To argue in reply that what the therapist needs is to acquire a thorough knowledge of the lower class individual, perhaps even to the extent of becoming a specialist with such patients (as Hollingshead and Redlich suggest), does not comprehend the issue. While this may seem an obvious and desirable step, there remains the possibility that equipping the therapist with conventional principles of practice may functionally disqualify him from success. It may be, of course, that special *principles* will be developed as guides to the psychotherapeutic treatment of lower class persons, but this requires an active research process the like of which is nowhere evident at the moment. Even so the possibility does remain that psychotherapy, as such, *is* the principles which govern it and that such principles are fundamentally inapplicable to lower-class persons among others. They may, for example, be wholly unable to "empathize" with the basic interpersonal aspects of the psychotherapeutic process and even less with its implicit moral, epistemological and operational precepts. In short, the time appears to have arrived when we must consider the possibility that psychotherapy, at least as presently constituted, is a treatment process the efficacy of which is confined to middle and higher class patient populations.[4]

[4] Hollingshead and Redlich offer some evidence which indirectly supports this point. Even when a deliberate effort was made to overcome the economic barrier (by free treatment) it was still extremely difficult to interest lower class patients in psychotherapy. Further, the high drop-out rate of lower

Along these lines it is necessary to recognize that the kinds of ethical and conceptual systems characterizing a people are not wholly arbitrary. They are rooted in the conditions of life under which they must operate and may, in general at least, be quite functional in respect to adjustment to those conditions (*cf.*, 3, 7). Instructing or indoctrinating them in other kinds of value orientations, *etc.* (as in psychotherapy) without due regard for the relationships between these orientations and real conditions of life may lead to more harm than good as the new orientations may be patently non-functional for those conditions. Thus the hypothesis presents itself that efforts to impose psychotherapy upon lower class patients and/or attempts to propagandize them into greater receptivity to such programs may in the long run, be creating problems instead of solving them. It may be "doing good" in the most pernicious sense of the term. Such an hypothesis must be entertained in the absence of satisfactory evidence to the contrary.

We might point out in conclusion that no recommendation is here being made that the possibility of applying some form of psychotherapy more widely to lower class patient populations be abandoned. Rather what is required are diligent efforts at determining the feasibility of such application in the light of the considerations outlined above. Whatever the outcome, it might be borne in mind that there is no disgrace in restricting the practice of psychotherapy to certain segments of the overall patient population where it can be shown to be effective. Specialized forms of treatment may be required for different populations; there is nothing especially novel about that. What would then be needed would be the development of alternative forms of treatment for alternative groups.

class patients undergoing outpatient psychotherapeutic treatment is notorious. Regarding this last point as yet unpublished data on 170 cases collected by the author at the William Greenleaf Eliot Division of Child Psychiatry in Washington University revealed that while some 70% of middle and higher class patients seen at the Division's Child Guidance Clinic progressed from the intake phase into full treatment, less than 30% of working class patients did so. Moreover, about 25% of higher class patients completed therapy as compared with only 9% of lower class patients.

Summary

From a survey of the recent literature concerning the prevalence and treatment of mental illnesses it was concluded that great caution should be exercised in the generalization of clinical data not only to "normal" populations, but even within clinical groups. The hypothesis was offered in regard to psychotherapy that it may well be a "middle class" form of treatment. In concluding it is suggested that clinical practitioners assume a far more studied and sophisticated cross-cultural (or, more exactly, pan-cultural) posture than has characterized them in the past.

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HOMOSEXUALITY AND PARANOID SCHIZOPHRENIA: A SURVEY OF 150 CASES AND CONTROLS

FRANKLIN S. KLAF, M.D., AND CHARLES A. DAVIS, M.D.[1]

In his analysis of Dr. Schreber's autobiography, Freud suggested that paranoid psychotic symptoms develop as a defense against emerging unconscious homosexual wishes. This hypothesis has generally been regarded as proven, but few scientific studies have been done to verify it.

Modern logic has taught us that, as Morris R. Cohen(2) puts it, "Those who begin with absolute truth cannot improve upon it." Unfortunately, on the basis of a few non-controlled observations, Freud's paranoia hypothesis was accepted as absolute truth, although Freud himself(4) cautioned against this, writing skeptically,

> It remains for the future to decide whether there is more delusion in my theory than I should like to admit, or whether there is more truth in Schreber's delusion than other people are as yet prepared to believe.

Modern logic has also taught us 3 important principles about hypotheses like Freud's. First, scientific investigation cannot prove such an hypothesis to be absolutely true, but only to be better than others in the field. Second, the real meaning of any hypothesis resides in its consequences. Third, the implications of an hypothesis should be considered independently of the question of whether it is in fact true. That is to say, although an hypothesis may be false, it may have useful determinate consequences.

Applying these principles to Freud's hypothesis, we see, first, that it would be impossible to design a study the results of which would prove the hypothesis to be absolutely true. According to the third principle, it really makes little difference whether the original hypothesis is true, since, even if it is not true, we still may obtain useful data by investigating its consequences. Therefore, we shall follow the second principle and investigate the meaning of Freud's hypothesis in its implications and consequences.

The process of investigating the consequences of a hypothesis is termed "verification." This process requires: first, that we deduce the consequences of the hypothesis; and second, that we examine these deduced consequences to see whether they agree with the hypothesis. Freud's hypothesis states that during an acute paranoid psychotic illness, a relative failure of repression occurs and repressed material comes closer to consciousness; paranoid symptoms develop as a defense against the emergence of unconscious homosexual wishes.

The first consequence deduced from Freud's hypothesis is as follows: Since unconscious homosexual wishes are emerging during the acute illness, we should expect to find such patients preoccupied with homosexual thoughts and wishes. With failing repression the histories obtained from these patients might more frequently contain evidence of previous homosexual experiences.

A second consequence of the Freudian hypothesis concerns the content of the paranoid delusions and hallucinations. Since the sexual problem is theorized to be of paramount importance as the basis of paranoia, we should expect the delusions and hallucinations to have prominent sexual content.

A third consequence concerns the sex of the persecutor. Freud states(4),

> The person who is now hated and feared as a persecutor was at one time loved and honored. . . . It is a remarkable fact that the familiar principal forms of paranoia can all be represented as contradictions of the single proposition "I (a man) love him (a man)."

Since the persecutor was previously the homosexual love object, we should expect the sex of the persecutor to be the same as that of the patient.

A fourth consequence requires clarifi-

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cation. Modern concepts of logic and operationalism have shown that facts and theories are meaningless except as parts of a system. Thus, Freud's paranoia hypothesis is an integral part of his entire system explaining psychodynamic development and functioning. In the Schreber analysis, Freud relates Dr. Schreber's religious preoccupation to Schreber's disturbed relationship with his famous and punitive father. Freud discusses elsewhere (5) the relationship between the domineering ego ideal and the development of a pathological propensity for religious belief. Thus, the role of the strict father in the development of pathological religious ideas and in the determination of actual and fantasied homosexual object choice is constantly stressed. Consequently, we should expect religious preoccupation to be close to the surface and freely expressed by many acutely ill patients, especially those whose premorbid religion was characterized by strong repression.

We shall examine the above deduced consequences to see whether they agree with or contradict Freud's basic hypothesis, and his hypothesis as expressed in his system.

MATERIAL

The material is derived from the 1943-57 psychiatric case records of the U. S. Public Health Service Hospital, Ft. Worth, Texas. During and after the Second World War, the hospital was a center for the treatment of servicemen (Navy, Marine Corps, Coast Guardsmen and Veterans) suffering from the major mental illnesses. Most of the patients were seen in the acute stage of their first episode of mental illness.

METHOD

From these psychiatric case records, 150 male cases diagnosed paranoid schizophrenia, and a control group of 150 male non-psychotic cases of other miscellaneous diagnoses (Table 1) were selected. In selecting the cases, the entire chart of each patient was reviewed. If there was any doubt concerning the diagnosis, the case was excluded from the study. Only patients conforming to the criteria of the American Psychiatric Association Diagnostic and Statistical Manual of Mental Disorders for the diagnosis of paranoid schizophrenia were included in the paranoid group. The non-psychotic cases were scattered among the 3 diagnostic categories listed in Table I. The main selection factor used for both groups was the presence in each record of an "adequate sexual history." By "adequate sexual history" we mean the notation in the psychiatric evaluation of each patient of a heterosexual and homosexual history. A definite recorded statement by the patient of the presence or absence of previous homosexual experiences was required for both groups in the study. Records containing only opinions of the examiner regarding the presence of latent homosexuality or the patient's inability to express concern about homosexuality were discarded. Many of the records, complete in other respects, did not meet these specifications and could not be included. An observation made during the perusal of the records was that the various examiners seemed to be divided into two main groups, the first seemingly trained to seek out homosexuality as the underlying factor in the paranoid schizophrenic process, and the second group, who

TABLE 1

DIAGNOSTIC CATEGORIES IN CONTROL GROUP

Psychoneurotic Disorders (includes former category "Mixed Psychoneurosis")	*Personality Disorders* (Includes former category "Psychopathic Personality." Paranoid Personalities were excluded.)	*Transient Situational Personality Disorders*
101	42	7
67.3%	28%	4.7%

seemed reluctant to take a complete sexual history on acutely ill psychotic patients. A typical comment of the second type of examiner was, "The patient was not questioned in this sphere (homosexuality) for fear of further shattering his defenses."

The following factors were selected for statistical evaluation:

1. *Age.*
2. *Marital Status.* The patient was placed in the married group if he had ever been married. No distinction was made for divorced or separated patients.
3. *Religion.*
4. *Previous Homosexual Experiences*—by which we mean the report of one or more overt homosexual experiences after puberty, *i.e.*, oral or anal sexual relations between individuals of the same sex, or mutual masturbation between individuals of the same sex.
5. *Presence of Homosexual Preoccupations During the Illness*—were recorded as present if homosexuality was reported as the predominant concern of the patient's verbalizations.
6. *Presence of Delusions or Hallucinations of Sexual Content During the Illness.* It is to be noted that this category is termed "Sexual," rather than "Homosexual," as it includes delusions and hallucinations regarding infidelity.
7. *Presence of Religious Preoccupations During the Illness*—recorded as present, if concern with religion was emphasized by the patient in his verbalizations.
8. *Sex of the Persecutor.* Often the patient's persecutor was not a specific individual, but was stated to be a group, *e.g.*, the officers in the Navy or the Communist Party. In such cases, the patient's persecutor is listed as male.

DISCUSSION

No comparison of the results of this study with those of previous studies will be attempted because of differing criteria used to define homosexuality and homosexual experiences and the absence of control groups in most other papers. The data compiled from this case study are summarized in Tables 2, 3 and 4. Tables 2 and 3 show comparisons of the collected data of the control and paranoid schizophrenic groups. Table 4 includes data on the persecutor within the paranoid schizophrenic group.

ADVANTAGES OF THE STUDY

1. The material was drawn from psychiatric reports that were not specifically designed for this study, thus reducing bias. The presence of an "adequate sexual history" was the main selection factor used. 2. Only paranoid schizophrenic cases conforming to the official diagnostic criteria were used. All of these patients had delusions of persecution. 3. This study, with the exception of two small Rorschach studies by Chapman and Reese(3) and Aronson(1), represents the first psychiatric study on this subject to include a control group. 4. The scope of recorded data in the various categories of the study is specifically defined. This is in marked contrast to previous studies, such as Gardner's(6), where a broad criterion like "Symbolism in action or words" is recorded as evidence of homosexuality, and Norman's(7), where

TABLE 2

	Paranoid Schizophrenic	Controls
Previous Homosexual Experiences*	55 36.7%	28 18.7%
Homosexual Preoccupations	46 30.7%	9 6%
Delusions or Hallucinations of Sexual Content	40 26.7%	0
Religious Preoccupations	33 22%	4 2.7%

* *Within the Paranoid Schizophrenic Group.*

1. 40% of those who had delusions or hallucinations of sexual content had previous homosexual experience.
2. 50% of those who had homosexual preoccupations had previous homosexual experience.

fear of sexual relations with women is taken as evidence of conscious homosexuality.

LIMITATIONS OF THE STUDY

1. The material is secondary source material. Further verification of Freud's theory and its consequences would require study of a large sample of primary source material. 2. The material used was obtained by several physicians with varying degrees of psychiatric experience, under the supervision of more experienced psychiatrists.

TABLE 3

	Age (Avg.)	*Marital Status*	*Religion*	*Prev. Homosexual Experiences*	*Homosexual Preoccupations*	*Delusions or Hallucinations*	*Religious Preoccupations*	*Age (Avg.)*	*Marital Status*	*Religion*	*Prev. Homosexual Experiences*	*Homosexual Preoccupations*	*Delusions or Hallucinations*	*Religious Preoccupations*
	CONTROLS							PARANOID SCHIZOPHRENIA						
Age	31.8				27.3	0	24.7	32.4				31.9	32.7	32.0
Married		89 59%		12 13.5%	4 4.5%		1 1.2%		71 47%		23 32.4%	20 28.2%	15 21.1%	17 23.9%
Single		61 41%		16 26.2%	6 9.8%		3 4.9%		79 53%		32 40.5%	26 32.7%	25 31.6%	16 20.3%
Catholic			15 10%	5 33.3%	3 20%		1 6.7%			34 22.7%	16 47.1%	10 29.4%	13 32.5%	8 23.5%
Protestant			126 84%	21 16.7%	6 4.8%		3 2.4%			116 77.3%	39 33.6%	36 31.1%	27 67.5%	25 21.6%
Jewish			2 1.3%	0	0		0			0				
No religion			7 4.6%	2 28.6%	1 14.3%		0			0				

TABLE 4

DATA ON PERSECUTOR IN PARANOID SCHIZOPHRENIA GROUP
(NONE IN CONTROLS)

Persecutor	*Total Number and Per Cent*	*Married*	*Single*	*Previous Homosexual Experiences*	*Homosexual Preoccupations*	*Delusions and Hallucinations*	*Religious Preoccupations*
Female Only	8 5.3%	8 100%	0	3 5.4%	2 4.3%	3 7.5%	1 3.0%
Male and Female	15 10%	6 40%	9 60%	5 9.1%	5 10.9%	6 15%	3 10%
Male Only	127 84.7%	56 44.1%	71 55.9%	47 85.5%	39 84.8%	31 77.5%	29 87.0%

This has been compensated for by a clear definition of terms and a careful review of the case protocols. 3. As is true in all psychiatric case studies, certain data may be influenced by the impressions of the examiner. In this paper, the recording of the presence or absence of homosexual or religious preoccupations may, to some extent, be so determined.

RESULTS AND THEIR RELATIONSHIP TO THE HYPOTHESIS

The first consequence of Freud's hypothesis was that we should expect to find acutely ill psychotic patients preoccupied with homosexual thoughts and wishes. In the paranoid schizophrenic group, 23 (41.8%) had homosexual preoccupations during the illness, as compared with 9 (6%) in the control group. This difference was found to be very significant using the Chi Square Test of significance. (A divergence as large as the one noted could have happened by chance alone less than one time in a hundred.) The first consequence was thus verified, since homosexual preoccupations during the illness were recorded approximately seven times as frequently in the paranoid psychotic group as in the control group. It was also proposed that, as a result of shattered defenses, it might be easier to obtain a history of previous homosexual experiences from the psychotic group. This was found to be the case. Previous homosexual experiences were recorded 1.96 times, or nearly twice as frequently in the paranoid psychotic group as in the control group. This difference was also found to be very significant using the Chi Square Test.

The second consequence noted was that we should expect the delusions and hallucinations of the paranoid group to have prominent sexual content. No comparison is possible with the control group, since non-psychotics do not have delusions and hallucinations. Within the paranoid group, only 40 or 26.7% had delusions and hallucinations of sexual content, including delusions of infidelity. Thus, the second consequence was not found to be verified by the study.

The third consequence of Freud's hypothesis was that since the persecutor was supposedly the homosexual love object, we should expect the sex of the persecutor to be the same as that of the patient. This was found to be as predicted. Within the paranoid group, 127 (84.7%) had male persecutors, 8 (5.3%) had female persecutors; and 15 (10%) had persecutors of both sexes.

The fourth consequence was that we should expect religious preoccupations to be expressed by many acutely ill psychotic patients. This was verified by the study. In the paranoid schizophrenic group, 33 (22%) had religious preoccupations during the illness, as compared with 4 (2%) in the control group. Thus, religious preoccupations during the illness were recorded approximately 8 times as frequently in the paranoid as in the control group. This difference was found to be very significant using the Chi Square Test.

SUMMARY

In this paper, the data obtained from a study of the records of 150 paranoid schizophrenic patients and a control group of 150 non-psychotic patients were presented and discussed in relation to Freud's hypothesis concerning the development of paranoid symptoms. Four consequences of Freud's hypothesis were deduced. Three of the deduced consequences received strong verification from the study, the differences between the paranoid psychotic and control groups being found very significant. The fourth consequence, that we should expect the final delusions and hallucinations of the paranoid group to have prominent sexual content, did not receive verification from the study. Comparison with the control group here was impossible due to the absence of delusions in the control group.

Another point needs to be mentioned concerning the present study. This is the fact that two trends may exist together in a personality and yet not necessarily be related. Bleuler originally brought up this point in commenting that homosexuality was very prominent in Schreber's case history, but may not have been the determining factor in the paranoid illness. While the present study, within its limits, lends strong verification to three consequences of Freud's theory, it is possible that future investigation may show the coexistence of

the two trends of paranoia and homosexuality to be a coincidental finding.

Few psychiatrists dispute that Freud's fecund intelligence was productive of many theories that have deepened our knowledge of psychological functioning. But, it is also a logical fallacy to argue that a theory is verified because it explains certain facts.

The process of verification, as utilized in this paper, is the same method used by the vast majority of scientific investigators.

We feel that the following studies of this important subject are needed :

1. Studies of the relationship of homosexuality to paranoid schizophrenia in female groups, as compared with control groups. As noted in the literature review, there are only two non-psychoanalytic case reports dealing with homosexuality and paranoid schizophrenia in females.

2. Studies on this subject utilizing primary source material. A protocol should be drawn up in advance, including definition of terms and categories to be recorded. This protocol should be used in interviewing a random sampling of paranoid schizophrenic patients, and a similar group of non-psychotic patients of varying diagnoses. Other control groups may also be used. The examiners should be free of preconceived opinions regarding the relationship of homosexuality to the paranoid schizophrenic process.

Until more scientific studies are made and analyzed, the hypothesis that paranoid psychotic symptoms develop as a defense against emerging unconscious homosexual wishes cannot be regarded as verified.

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THE CULTURAL PROBLEM : PSYCHOPATHOLOGY IN TAHITI

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French Oceania comprises several groups of Islands in the South Pacific lying between 5 deg. and 30 deg. south latitude and 130 deg. to 155 deg. west longitude. The best known is Tahiti, which lies about 3600 miles SSW of San Francisco, but the area includes all the Windward Islands, the Leeward Islands, and the Marquesas, Tuamotus, and Australes with a total population of 73,201 (1956), about 87% Polynesian and 9% Chinese. The popular myth that there are 5 women to every man, as stated by an American who had cruised among the islands for over a year, is quite out of line with the official census figures which actually show a slight preponderance of men in the area.

The principal medical facilities for the area are in Tahiti on the outskirts of Papeete, the capital (17,247). These include the Department of Public Health, the Civil Hospital, and a separate small "asylum" of 17 rooms. Tahiti was proclaimed part of the French Republic in 1880, and the first recorded psychiatric patient was hospitalized on September 18, 1884, an alcoholic homicide who was released for good behavior on January 20, 1886. The original psychiatric ordinance dates from December 28, 1885, and as is usual in French colonies, was based on the Law of June 30, 1838. A new Ordinance was passed on August 28, 1913. The present building, the usual structure of the older period with stone cells, iron bars, enormous clanking locks, and no plumbing, became the asylum in 1948. The records have been rather casual until quite recently, but there were 8 names on the books in 1911, all Tahitian ; and on January 1, 1947, there were 13 names, 4 of them admitted during 1946 ; 8 additional names occurred in the notes during 1947. At that time there was one patient who had been hospitalized since 1926, and another who had been admitted for the third time in 1931.

Firm figures begin in 1954, when there were 31 patients seen with psychiatric diagnoses, of whom 18 were hospitalized ; in 1956 these figures were 57 and 46. This represents an increase from 1954 to 1956 of 84% in patients seen, and 155% in patients hospitalized. Since the total population only increased 16% from 1951 to 1956, the psychiatric situation may be taken as a dramatic illustration of the principal that hospital figures represent merely the outflow of psychopathology from a previously existing reservoir of unknown extent, and that arrivals at the hospital depend upon the quality of the care offered and the state of public opinion rather than on the incidence or prevalence of mental disorders. The fact that accurate record-keeping was initiated is taken to indicate that the quality of medical care suddenly improved in 1954.

At the time of my visit in May, 1958, there were 3 physicians at the hospital : the Chief, Dr. Georges Thooris ; Dr. Guy Ruez ; and Dr. Henri Fayet. They were fully occupied with communicable diseases, acute surgical problems, and other urgent matters. None of them had been trained in psychiatry, and they had no facilities available for treating psychotics, not even an electroshock apparatus. By the time I was ready to depart, however, they had the idea of attempting cardiazol shock treatment with some of the chronic patients who had been in custodial care for 10 years or more. The character of these colleagues is demonstrated by the fact that Dr. Fayet had for a house-boy a 15-year-old juvenile delinquent who had been remanded to him by the Court, and was going through the initial throes of domiciliary placement with this boy. He had previously accepted another such individual who, after a few years in the Fayet household, had settled down to an honest and respectable life in the community. The existence of such juvenile delinquents is in itself a commentary on the cultural problem : the fact that it is part of the Tahitian "culture" to distribute offspring among uncles, aunts and other collaterals for upbringing does not necessarily make this kind of relegation

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more acceptable to a Tahitian child than it would be to an American child.

The staff was kind enough to allow me to interview all 12 patients in the "asylum." There were 3 others in the general hospital whom they requested me to interview for diagnostic purposes. The patients in the asylum are listed below.

The diagnoses, except in 2 cases, were not difficult to make, and in no case were "cultural" factors significant in this respect. The interviews were conducted in French by the writer, with occasional help from a Tahitian nurse who acted as interpreter.

Tham (No. 1), Loc (No. 2), and Tupai (No. 10) exhibited hebephrenic mannerisms and muttering. Faaua (No. 3), Amona (No. 6), and Ivaa (No. 8) remained mute in fixed attitudes and were easily recognizable as catatonics. Mata (No. 4) decorated herself with flowers and had manic outbursts with hypersexuality. Rich (No. 5) sat in a depressive attitude listening to hallucinations to which she responded dejectedly. Parau (No. 7) had catatonic outbursts of violence. Elie (No. 9) talked about Presidents and Generals, and said he had millions of children. He kept asking to go to the maternity ward. His paranoid delusions became quite clear.

The two diagnostic problems were Moana (No. 11) and Kong (No. 12). Moana was subject to outbursts of violence, and when he felt one coming on he asked to be locked up. There was some doubt as to the nature of these outbursts, and psychomotor epilepsy could not be ruled out without laboratory studies which were not available. Kong exhibited bizarre behavior. He rode a bicycle into church and had some ideas of reference. It appeared that prolonged observation and careful roentgenographic studies would be required to rule out one of the pre-senile dementias, such as Pick's disease.

The interviews with the 3 patients at the general hospital were more detailed. The difficulties in these cases did not lie in the cultural situation, but in the clinical psychiatric problems.

1. Monique was a 23-year-old Tahitian housewife whose chart showed nothing remarkable in the physical examination. The Kahn was negative, with normal urine, spinal fluid, and blood cholesterol. The hematological findings were as follows : r.b.c. 3.8 million ; hemoglobin 65% ; w.b.c. 6600 with 8 eosinophiles ; these were considered by the staff to be within normal limits for the region because of the endemic parasites.

The patient sat trembling with fear, her pulse 100, when the visitor was introduced. He proceeded as he would have during a diagnostic interview with such a patient anywhere. First he allowed her to present some complaints, which in this case were principally bitemporal headaches, for which her mother had advised her to go to the hospital. Then he did a partial neurological examination so that she would see that he had a familiar medical interest in her condition. This revealed little except equally dilated pupils and a coated tongue.

In the course of the examination, he asked Monique if she wrote with her right hand or her left. She replied "Both." He then

	Name	*Sex*	*Age*	*Origin*	*Date of admission*	*Diagnosis*
						(S. = Schizophrenia)
1.	Tham	F	36	Tahiti	3/49	S. Hebephrenic
2.	Loc	F	46	Indo-China	2/40	S. Hebephrenic
3.	Faaua	F	48	Tahiti	1/41	S. Catatonic
4.	Mata	F	51	Moorea	3/40	Manic
5.	Rich	F	53	Tuamotu	1/26	Manic-Depressive, Depressed
6.	Amona	M	28	Tubuai	1/51	S. Catatonic
7.	Parau	M	29	Tahiti	11/49	S. Catatonic
8.	Ivaa	M	33	Tuamotu	12/55	S. Catatonic
9.	Elie	M	36	Tahiti	3/51	S. Paranoid
10.	Tupai	M	38	Tahiti	9/56	S. Hebephrenic
11.	Moana	M	49	Tuamotu	4/46	S. Catatonic or Epilepsy Psychomotor
12.	Kong	M	55	China	1/57	Paranoid or Pre-Senile Dementia

offered her writing materials and she responded first with some scribblings and later as free composition wrote her name and birthdate and some religious sentiments. This set the stage for the first psychiatric question : "Do you have any children ?" She said that she had 3, and that one was with her mother. The doctor asked about the other two and this started tears, so she was brought back to the paper and asked to draw a man, a woman, and a house. She refused the woman at first ; and later, when requested, she steadfastly refused to draw people without clothes. She evaded discussion of the drawings by talking about fishing, canoeing and religion, and the psychiatrist went along with this. The lack of pressure putting her more at ease, she was now asked if she were hearing voices. She had a complete set of these : a parental voice saying that God would forgive her sins, a matter-of-fact voice that told her to eat well, and a wicked little devil. She then recounted a dream of hands grabbing her : bad women.

The question of the other two children was approached once more. She said they had been miscarriages, and began to weep again. The bad voices were tempting her at this point and she brought up her religious mania to ward them off. By this time she was fully co-operative and did not want to terminate the interview, saying that she wanted to go on talking. She talked about serpents biting her in the foot, big serpents like one sees in the movies, and pointed to an ulcer on her foot. The psychiatrist asked the local doctor if there were any snakes in Tahiti and he said there were none.

At this point the patient was ready to be turned over to the local doctor for psychotherapy, since she was now eager to talk. A firm diagnosis was not yet possible, but the picture was clearly a hysteroid one, probably of schizophrenic origin. The solution for both diagnosis and treatment, he was told, was to listen, listen again, and listen some more. He said he did not have the time available and wanted advice concerning drug therapy.

This patient's behavior and attitudes were more childlike than those of most human beings of her age and experience. Allowances were made for this and she was treated like a girl of high school age. Cultural factors had no more clinical relevancy than the "cultural" problems of an American high school corridor, although they were radically different. In summary, this young woman was suffering from a familiar form of psychiatric disturbance which could be diagnosed and handled on the basis of foreign experience.

2. Pierrette, a 20-year-old woman, had been brought to the hospital by her parents that afternoon. She stated that she had become enraged because they took away her baby and told her it was dead and then she had found out it was not so. Pierrette was her mother's fifteenth child and had been given to the grandparents to raise in the Leeward Islands while her parents remained in Tahiti. Initially she said that she had first been married at 16, that she had had one son and then a miscarriage, and that she had left this husband because he beat her. Then she had had a miscarriage with her second husband, and afterward the child who had been taken from her. Later she changed her story. She said that her first baby died because of poor midwifery, her second had gone with his father, her third had also died because of a careless midwife, and the fourth had been taken from her.

The interview was a stormy one. The patient showed considerable hostility toward the visitor, and if she was pressed became angry and finally wept. After testing these responses for a few minutes, the psychiatrist thought it was indicated to tell the patient that he was a neutral medical man and not a friend of her mother's. He also told her she could depart if she so desired. These maneuvres were successful and she became more at ease and more co-operative, in spite of her feeling that people were trying to make fun of her and to make her cry. She then admitted that she was hearing voices and that it was these voices that made her so hostile.

At this point her anger had been turned aside from the psychiatrist and she became venomously angry at Dr. Ruez, who with Dr. Fayet and the Tahitian nurse-interpreter, was also present. The psychiatrist asked the interpreter if this kind of anger was unusual among Tahitians and she replied that it was. He then asked Pierrette how she felt about her mother sending her to the grandparents. Pierrette did not understand this question, and the nurse explained to the doctor that this was quite a usual occurrence and that Tahitian children were accustomed to it. The psychiatrist replied that this was interesting, but politely insisted that the nurse nevertheless ask Pierrette in Tahitian how she felt about it. Her question caused a renewed outburst of rage on the part of Pierrette and it was evident, without understanding what she was saying, that she was very angry at her mother. In fact,

the real occasion for bringing her to the hospital, it now appeared, was that she had taken a knife to her mother.

The disposition of this case posed a difficult problem. Pierrette was too disturbed to be kept long in the general hospital. On the other hand, all present agreed that to put her in the "asylum" under present conditions might be equivalent to a very long sentence of confinement. But if she were released, she might hurt someone, since she was an acute schizophrenic under the influence of voices. It was decided to try metrazol treatment.

3. The third case was that of Mou, a 20-year-old man accused of having had intercourse with a 10-year-old girl. He had originally been charged with the French equivalent of statutory rape, but this charge had been reduced because the prosecutor felt that he might be acquitted of rape and there would be more chance of conviction if the charge were less serious. The judge had remanded him to the hospital for psychiatric examination.

The mother, a Tahitian-born Chinese, had been married 3 times, and had 11 children. As a girl she had married a 27-year-old man from China, by whom she had had 3 children. The patient was the third of these, and when he was two years old, his father had died. The following year the mother remarried a man of her own age, had another child, and was widowed again when the patient was 5. Another year elapsed and she married another man from China who was 22 years older than herself, by whom she had 7 children. Thus there were 13 people living in the house. The patient slept in the same room as his parents until he was 10, and then had been moved in with his sisters, and was particularly attached to a half-sister who was 4 years younger than himself. It was felt that this was of some significance, since she had been 10, the same age as the alleged victim, when the patient entered active puberty.

The plaintiff had lived next door to the patient for many years, with her parents and some married sisters. Mou stated that one day when he was down by the river he had run across the girl and she had made some seductive advances. He had responded and they had ended up having intercourse. She said nothing about it to anyone until some other children who had observed the occasion reported it to her mother, and then she had confessed, with the result that charges were made and pressed.

Mou said that he had had about two litres of wine on Saturday and two more on Sunday, but none since, and the alleged offence was committed on Tuesday. He had had occasional intercourse with girls his own age and spent much time in erotic fantasy. He had recently had frequent spontaneous ejaculations during sleep, and careful enquiry elicited that these were unaccompanied by any dreams that he could remember, in spite of their frequency. The psychiatrist made a diagnosis of infectious prostatitis on the basis of this history, and suggested that Dr. Ruez enquire about this. The patient then stated that he had had untreated gonorrhoea a year previously, *i.e.*, gonorrhoea treated by a native herb doctor. His prostate was now massaged. It was found to be slightly enlarged and yielded a large spurt of purulent fluid.

The medico-legal diagnosis was then made as follows: (a) Legally sane. (b) Sexual psychopathy, mild. (c) Prostatitis.

It was recommended that effective treatment for his gonorrhoea be initiated, with prostatic massage, and that following this, before trial, the psychiatric examination be repeated.

The significant features which emerged from this survey of the available psychopathology were as follows:

1. *Epidemiology.* The very large percentage increase in psychiatric patients seen, after the medical facilities were slightly improved, is noteworthy. From experiences in other countries(1) it would be expected that a further large increase would occur if further improvements were made. After adventitious social contact with the populace, there is no reason to doubt the existence of a large reservoir of psychopathology in Papeete at least. This belief is confirmed by Dr. Thooris' impression that Polynesians are very unstable and suffer outbreaks of nervous disorders with slight provocation, especially manifested by skin pathology. The hospital admissions for psychiatric disorders more than doubled between 1954 and 1956, and it is not difficult to conceive that the latter figure of 46 admissions could easily triple. This would yield a rate of about 2/1000 population admitted for psychiatric disorders per year, which is within the expected range. That is the rate for Martinique, a well-equipped French colony (1954), and

it lies between the U. S. rate of 1.36/1000 in 1935 and 2.34/1000 in 1956(2). There is as yet no reliable evidence that the incidence or prevalence of psychiatric disorders varies in different parts of the world or in different cultural and racial groups ; the evidence favors rather differences in the tendency to seek treatment(3).

2. *Etiology.* The cases cited indicate that certain situations tend to have a disturbing influence on sensitive personalities, regardless of cultural sanctions or freedoms. A child who is sent away by her mother may feel just as rejected in a society which has a relaxed attitude about child-bearing and child-disposition as in a society which is very rigid about such matters. The fact is that in pre-missionary days it was a Polynesian custom to throw extra children into the sea(4), and it is difficult not to interpret the present relaxation as a politer continuation of this decisive form of rejection. Such drownings were not any less traumatic to the children concerned because they were culturally sanctioned. And a boy who grows up in a crowded household in a society whose sexual freedom is still publicized (its complexity can be judged by Monique's prudery about drawing nudes) may be just as disturbed by too much contact with sisters and parents as a boy who grows up in the slums of a Puritan city. Psychoanalysts all over the world tend to confirm the universality of the traumatic effect of certain experiences on certain kinds of people regardless of the attitude of the surrounding "culture."

3. *Symptomatology.* The symptomatology of psychoses and borderline states can be recognized by an experienced clinician even when he moves from culture to culture without special preparation.

4. *Diagnosis.* Again, psychiatric diagnoses can be made on medical grounds which are essentially independent of local cultures, as demonstrated by the hospital cases in Tahiti.

5. *Therapy.* Psychotherapeutic maneuvers can be readily transferred from one culture to another. Principles learned in the treatment of young women in Connecticut or California were just as effective in the South Pacific.

Once the patient arrives in the clinical situation, therefore, cultural considerations seem to be of little moment. Local prejudices are of great importance administratively, politically, sociologically, and economically, but there is no evidence that they are of psychiatric significance at the hospital level.

Generally speaking, attempts to relate so-called "cultural" factors to mental illness are open to question. The current tendency in this direction may be obscuring more important issues. A careful reading of the literature gives the impression that psychodynamically, culture is on the same level as autointoxication, racial prejudice, and economics. The patient may be only too relieved to blame his troubles on the intestinal system, the social system, or the economic system, and the biased therapist may help him along with this project of finding a scapegoat. The cultural approach encourages the nostalgic illusions(5) of most human beings : the pathetic fallacies of the Golden Age, the Blessed Isle, and the Favored Class. It is not likely that (a) "Things were better in the old days." There is no evidence that (b) "Things are better among (so-called) primitive people, such as those who live on tropical islands." And it is begging the question to say that (c) "Things are better among people who have fewer worries, such as Caucasians, Negroes, primitive people, educated people, rich people, peasants." The doctrine that people are victims of their environments is a doubtful ortho-psychiatric position.

It appears that a certain proportion of every population, and probably the same proportion of every large population, is going to suffer from outbreaks of psychosis or neurosis each year, regardless of background : Papuans, Creoles, Polynesians, Siamese, or Anglo-Saxons. This impression is based on visits to mental hospitals in 30 different countries, and consideration of the indicated reservoir of psychopathology in each place. The proportion of those afflicted who will come to the attention of the medical authorities under various conditions is another kind of problem, and hospital admission rates should be carefully scrutinized lest they distort the psychiatric realities. In any case, before the relationship between culture and psychiatric disorders can be

adequately evaluated, the concept of "culture" itself requires more rigorous clarification, and this is not a simple matter, as Morgenbesser points out in his article on the "Role and Status of Anthropological Theories"(6).

SUMMARY

This is the third of a series of papers on the psychiatry of the South Pacific. The present study includes all known hospitalized psychiatric cases in French Oceania, together with some historical notes. Cultural factors were of negligible significance at the clinical level. The current emphasis on such factors is interpreted as an attempt to find a successor to such scapegoats as devils, autointoxication, tubercle bacilli, economic conditions, *etc.*, as etiological agents in psychiatry. This emphasis is most likely an outcome of the nostalgic illusions of the Golden Age, the Blessed Isle, or the Favored Class, which was or is free of psychiatric disorders. Observations in 30 different countries indicate that the reservoir of psychopathology is of the same order in every large population the world over.

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MANIC-DEPRESSIVE ILLNESS IN ISRAEL

JOZEF PH. HES, M.D.[1]

On occasion of the 10th anniversary of the Talbieh psychiatric hospital we decided to survey all the manic-depressive patients hospitalised during the years of its existence. The first difficulty we met was in defining the concept manic-depressive illness. We could not include every psychiatric disorder accompanied by more or less severe mood swings, because few of them occur without any mood disturbance. The definition given by Mayer Gross, *et al.*(12) seemed too general: he includes, in addition to manic-depressive illness in the strict sense of the word, also involutional psychosis and the cyclothymic constitution. Involutional psychosis has a heredity and a pre-psychotic personality different from that of manic-depressive illness. We did not include cases of cyclothymic constitution because the leading symptomatology was different *e.g.*, schizophrenic. In our study the illness is characterised by mood swings occurring in waves and/or alternately, in the absence of symptoms such as hallucinations or personality deterioration. In our opinion the clinical picture was very important in selecting the patients. Each case possibly due to reactive factors or suspicious of schizophrenia was excluded.

One finds in the literature different and contradicting opinions concerning manic-depressive illness and its incidence in certain groups *e.g.*, Jews, women, upper social strata(8, 9, 12), Hutterites(4), Negroes (19) *etc.* Among native born Americans the disease appears to be rare in contradistinction to immigrants(16). In the older German psychiatric literature one finds the opinion that manic-depressive illness is a typically Jewish psychosis, a point defended in 1957 by Kalmus(7) in his study on data of Israeli patients.

Heredity plays an important part in the causation of the psychosis; however, Cohen, *et al.*(3) emphasize the part played by special social conditions *e.g.*, membership in a minority group, unusual economic status and particular illness.

[1] From Talbieh Psychiatric Hospital, maintained by Kupath Holim, (Workers Sick Fund) and affiliated with the Hebrew University Medical School and Hadassah, Jerusalem, Israel. Present address: 45 George St., East Haven, Conn.

PROCEDURE

In our study we investigated the incidence of manic-depressive illness in the hospital population from November 1949 to December 1958. We compared our results with data on the frequency of the disease in the whole country and with opinions occurring in the psychiatric literature.

We examined 2,684 records of first admission patients during the above mentioned period. Talbieh hospital contains 200 beds, 70 in the male wards and 130 in the female, and treats members of the Workers' Sick Fund from the Jerusalem area and from all over the country. This Sick Fund comprises 62.4% of the total population and includes employers as well as employees. The patients are Israel born, new immigrants and immigrants who entered the country before the establishment of the state in 1948. From each record we noted the name of the patient, country of origin, year of birth, year of immigration, sex, family status, data on heredity, particulars about the disease, its way of starting and the clinical picture.

RESULTS

We found that among 2,684 patients there were 100 manic-depressives according to our diagnostic criteria mentioned above; 62.6% were females, the rest males. Of the 100 manic-depressive patients 65% were females, the rest males: 12 were Israel born, 83 came from Europe and the Americas, 5 from Africa and Asia.

Table 1 gives the country of origin of the 2,684 patients.

Of the 88 immigrant patients, 70 entered the country before the establishment of the state in 1948, 16 came to Israel in 1948 and onwards. On two patients there were not sufficient data.

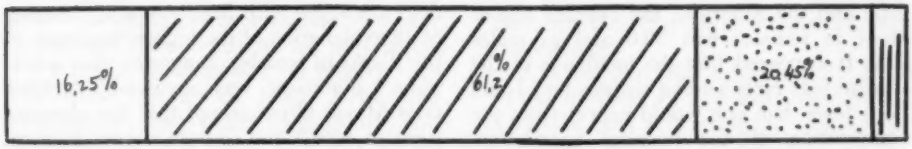
Data on heredity were rather incomplete

TABLE 1

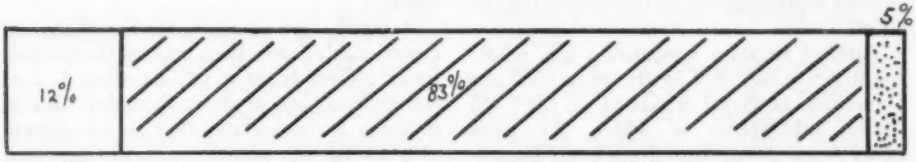
2,684 First Admissions in Talbieh Hospital During the Years 1949-1958 According to the Country of Origin

Country of origin	*Fem.*	*Male*	*Total*
	%	%	%
Israel born	9.4	6.85	16.25
Jews from Europe & Americas	41.0	20.2	61.2
Jews from Africa & Asia	11.1	9.35	20.45
Unknown	1.1	1.0	2.1
Total	62.6	37.4	100.0

GRAPH 1

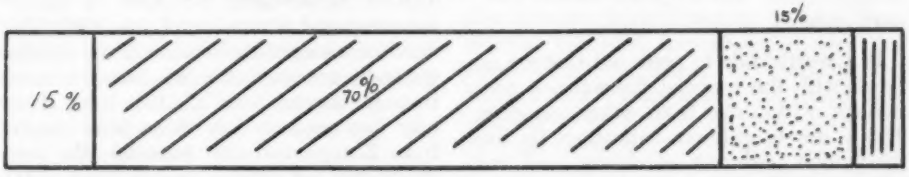


2,684 First Admissions in Talbieh Hospital During the Years 1949-1958 According to Country of Origin



100 Manic-Depressives in Talbieh Hospitalized During 1949-1958 According to Country of Origin

Jewish Population by Country of Birth 31 XII 1956



745 Manic-Depressives and Mixed Disease Discharged During 1957 From Psychiatric Institutions in Israel

as is almost always the case in an immigration country. If one considers the presence of mental disturbances in the families of the patients, also disorders which do not require hospitalisation, as a positive finding concerning heredity, then our results show among 100 patients, 52 victims of heredity. Negative findings were recorded in 30 cases and on 18 cases there were not sufficient data.

In eastern patients *i.e.*, those from Africa and Asia, the average age of onset in the case of the 4 females was 22.6 and of the only male patient 50 years.

In western patients *i.e.*, those from Europe and the Americas, the average age of onset in females was 30.0 and in males 29.9. It appeared that the incidence of the disease was rarer among eastern and Israel born Jews than one would expect from the composition of the hospital population. Likewise the disease was more frequent in western Jews than was expected according to their representation in the hospital population.

Turning to data concerning the whole country we find that in the year 1957 the population of Israel consisted of 1,667,455 Jewish inhabitants of which 35% were Israel born, 37.5% were Jews from Europe and the Americas and 27.5% were from Africa and Asia. The frequency of manic-depressive illness and mixed diseases was higher in the immigrants from Europe and the Americas. Of 745 patients discharged from mental institutions all over the country in 1957 there were 70% immigrants from Europe and the Americas whereas only 15% came from Asia and Africa. (See Graph 1.)

DISCUSSION

It is worthwhile to point out that we are faced with the especial difficulty of comparison with data from other countries. This is due to different indications for hospitalisation, different conditions and customs regarding hospitalisation of patients, differences between the several countries, between various periods and between diverse social classes. In the Hutterites, for example, exists a high morbidity against a very low hospitalisation rate as a result of the special socio-religious way of life in this community.

To the difficulty of defining the disease we referred already. In our investigation we examined only cases of manic-depressive illness in the strict sense of the word, whereas the data about Israel as a whole refer to manic-depressive combined with mixed disease and involutional psychosis.

Our results comprise new findings and others which are not outstanding such as the fact that among 100 manic-depressives there were 65 women. This finding is in conformity with data of the literature: Kraepelin(8) mentions 70% females, Kraines (9) 61.7%, Lundquist(11) 61.5%. Mayer Gross, *et al.* likewise mention this fact but does not agree with Rosanow who, "aiming at an explanation of the higher incidence of the illness in females, suggested that a sex-linked dominant was involved." Neither does Mayer Gross accept that the aberrant form in which the disease occurs in men, *e.g.*, recurrent alcoholism, can account for the whole of the differences between the sexes.

As to the age of onset, we found age 30 considerably lower than most of the authors found: Cassidy, *et al.*(2) mention 41.2 as the average age of onset in males, 41.4 in females. Kraines states that in his private practice the age of onset seems to be much lower. Patients paid their first visit at the age of 30-34; however, the first attack of manic-depressive illness started even at an age as low as 25-29 years, a finding which agrees with our results.

We found the incidence of the illness in Israel lower than the average incidence in the literature with the exception for Finland. Kraines(9) mentions an incidence of 4:1000, Mayer-Gross 3-4:1000. In Israel[2] we computed a number of 0.4:1000. This number is surprising because of the opinion generally accepted that the disease is more frequent among Jews(12, 13). Even if we take into account only those Jews coming from Europe and the Americas, the incidence does not surpass the rate of 0.8:1000.

How to explain the low morbidity in Israel? One has to take into account that the rate 0.4:1000 is related to hospital

[2] Basic data on patients discharged from psychiatric institutions 1957 by H. S. Halevi M.A., Asst.-Director-General (Planning) Ministry of Health, State of Israel, Jerusalem 1959.

cases only. Without doubt many patients did not receive institutional treatment because of shortage of beds. In addition no small number of depressives received ambulatory ECT. Roberts and Myers(17) who observed the decreasing morbidity of manic-depressive disease among U. S. Jews during the last decades, believe that "acculturation of the Jewish family to America, has tended to play down the accentuation of feeding" which, according to the authors, "has been an important factor in the causation of affective illness among Jews."

It is worth mentioning that Halperin(5) already in 1938 found that manic-depressive disease is more frequent among non-Jews in Palestine than in the Jewish population.

We found the disease rarer among Jews who have immigrated from Africa and Asia than in those coming from Europe and the Americas. It is stated in the literature that a low incidence occurs in Negroes of the Gold Coast(19), in rural Negroes in the U. S., in the Japanese and in the Kenya Africans(17). In relation to these observations we have to mention that the eastern Jews under the existing conditions in Israel, belong to the lower socio-economic classes. Modern technical changes did not penetrate into their communities as they did in western groups. It would be interesting to investigate in another few decades the incidence of the disease in eastern communities in order to find out what role acculturation played in causation.

It is possible that the high incidence in western Jews is explained by the facts put forward by Cohen, *et al.*(3). Cohen points out that in the patients treated by him and his co-workers "each family background was set apart by some factor which differentiated it from others in the surrounding milieu (membership in a minority group, unusual economic status, particular illness)." The patients "were expected to conform to high standards of behavior, based on the family's concept of what the neighbors required."

If these circumstances merit the importance Cohen attaches to them, we should expect then, a higher incidence among eastern Jews in Israel. But probably this group has been living too short a time under these particular circumstances to experience their influence.

An interesting study in this field(4) describes the Hutterites, a religious group of 9,000 people living in 70 collective settlements in the U. S. and Canada. These Hutterites are very tolerant towards mental patients. Among them is an extraordinarily high incidence of manic-depressive disease. Two circumstances which may offer an explanation are the following :

1. A high amount of inbreeding because marriage outside the group is forbidden. In consequence of this inbreeding one sees an accumulation of hereditary factors, which may favor the incidence of the disease.

2. Because of their religiosity the Hutterites suppress all aggressive tendencies. Likewise they refuse regular army service. It is possible that as a result of this suppression one sees the development of an extremely severe superego and accordingly more guilt feelings and depressions.

Pollock(16) in his study from 1930, found a higher incidence of affective disorders in immigrants than in native born Americans. Likewise we found a higher incidence in immigrants than in Israel born. The higher incidence in long-stay immigrants may be explained by the fact that immigration before 1948 consisted almost exclusively of European Jews. The great influx of immigrants from Asia and Africa into the country came about 1951.

Summary

The author surveyed 100 manic-depressive patients hospitalised in Talbieh Psychiatric Hospital, Jerusalem, during the years 1949-1958. These 100 patients are divided into 3 categories : 83% Jewish immigrants from Europe and the Americas, 5% from Asia and Africa and 12% Israel born. Seventy percent were long-time immigrants whereas 16% were newcomers.

Data were also presented about 745 manic-depressives, who were discharged from psychiatric institutions all over the country during 1957. The incidence of manic-depressive disease was compared with data from the psychiatric literature.

The incidence in Israel, according to the

number of discharged patients during the year 1957, was 0.4 :1000, whereas the incidence in the average population on the whole world is 3-4 :1000. The author presented hypotheses concerning this observation.

Conclusions

According to our data, manic-depressive illness is not more frequent among Israeli Jews than in any Gentile population. In Israel the incidence is higher in Jews coming from Europe and the Americas than in Jews from Africa and Asia.

Heredity plays an important part in causing the disease, however, and research in the field of anthropological and environmental factors seems very desirable.

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CHILD PSYCHIATRY FACILITIES IN MOSCOW, RUSSIA [1]

GEORGE H. KLUMPNER, M.D.[2]

On July 9, 1959, I visited a psychiatric hospital for children in Moscow, Russia, with an internist from Cornell Medical School. Our conversations were carried on through an excellent interpreter assigned by Intourist who made the arrangements for our visit.

The children's section of the hospital was located close to an adult mental hospital on the outskirts of Moscow. The architectural style of all the buildings was similar to that of the state hospitals built in this country during the 19th century, and I was surprised to learn that the children's hospital, which looked as if it could easily have been one hundred years old, was built in 1938 specifically as a children's psychiatric unit.

The patient-staff ratio was quite favorable. For 240 children there were 120 registered nurses, plus 25 student nurses, and 65 maintenance personnel. There were also 17 full time child psychiatrists. On the whole, the hospital had a gloomy atmosphere. Wards were large with 20 to 30 children in each ward. There were no facilities for the children's personal belongings. We wore white coats as we toured the hospital and were shown the kitchens, the wards, the dining rooms, and a darkened room where 20 children were receiving electro-sleep treatment.

Electro-sleep treatment is given to children with chorea and encephalitis, as well as to schizophrenic and neurotic children. Schizophrenics are also treated with vitamins and insulin. The neurotic children are not, as a rule, given insulin. In addition to these somatic treatments, the staff considered the ward experience to be therapeutic. They were very proud of their large garden in which the children could work as a therapeutic activity. They were also quite proud of their small zoo which included a donkey, foxes, and monkeys.

We spent over 3 hours with 7 Russian child psychiatrists, all women over 40 years of age. We asked if all child psychiatrists in Russia were women. They said this was true in Moscow, for they felt that women naturally liked children and consequently were well suited to the work. However, they said that all of the child psychiatrists in Leningrad were men, but offered no explanation for this odd fact.

The training of psychiatrists in Russia is somewhat different from our own. Following graduation from high school, they spend 6 years in medical school, and then work as general physicians in a clinic for 2 or 3 years. They next have 6 months training in adult psychiatry, after which they continue working in psychiatry. They return for training periods of about 5 months each 2 to 3 years thereafter.

The doctors with whom I spoke read regularly every issue of both *The American Journal of Psychiatry* and *The American Journal of Ortho-psychiatry*. They found the former much more helpful to them in their work with children. They felt that *Ortho* contradicted their interests through its psychoanalytic emphasis; however, they said they found *Ortho's* case descriptions interesting.

After touring the hospital, we were served tea, caviar sandwiches, and oranges in an outdoor pagoda. Here we talked for about two hours on the subject of neuroses in children and their treatment.

These doctors explained neuroses in accordance with Pavlovian theories, *i.e.*, that neurosis follows a psychological shock to the nervous system. Even a slight shock may be sufficient if the nervous system has been weakened. They frequently see children after some physical illness (or children whose physical development has been retarded) and they feel that these children are very susceptible to neuroses because their nerves have been weakened as a result of the disease process. In listening to them discuss various cases, it seemed that in every case they were able to elicit a history of earlier illness to which they attributed etiologic significance.

[1] Read at the meeting of Illinois Psychiatric Society, Nov. 18, 1959, Chicago, Ill.

[2] 642 Gunderson Avenue, Oak Park, Ill.

The presenting symptoms of some of their hospitalized neurotic children included sleep or speech disturbances, tics, reactive depressions, agitation, phobias, and stubbornness.

I experienced some difficulty getting the doctors to discuss their ideas about psychotherapy with children. In most instances, they said they used rather forceful, positive suggestion to the child two to three times weekly, and they not infrequently used hypnosis.

They presented a case of an 8-year-old girl, who was referred with a diagnosis of "mutism." At the time of admission she spoke in a low voice, and had the social maturity of a 6-year-old. She was the youngest child in the family and had been spoiled. She had 2 older sisters who had misbehaved and when she told her parents about their misbehavior, got them into trouble with the parents. Her sisters then beat her and shouted at her that she must not tell her parents when they did bad things. Following this experience, the child lost her voice. The staff felt that the nature of the symptom was over-determined because the girl had had a cold, with a sore throat at the time. This, they felt, determined the type of her symptoms.

The patient was seen daily on rounds and three times a week in private interviews lasting about 40 minutes, where suggestions and hypnosis were used. During the first interview the psychiatrist tried to convince the little girl that she should speak only with her doctor and it was only necessary to speak in a whisper. When she gradually began to follow these instructions, the doctor introduced her to other people to whom she could also speak. Her therapist applied some electricity to her larynx, so that she would be able to feel her tongue and the way in which it was supposed to work.

When this patient played with the other children in the ward, she would play that she was the teacher. Because of this tendency, the doctors contacted the child's teacher at school and worked out a plan whereby the little girl would be made an assistant teacher when she returned to school.

The patient had been in the hospital a little over a month. Her speech was becoming louder and she was now able to speak with anyone. She liked to read and was therefore assigned to read to two other children in the hospital. Such an assignment fitted in with the doctors' concept that everyone in the hospital should help the doctors.

The hospital regularly arranges programs in which the children perform for their parents on visiting days. After this patient had been in the hospital for a time, she said that she wished to recite a poem on such a program.

The psychiatrists' work with the child's parents was essentially manipulative. Normally, parents were allowed to visit their children twice each week, but with this particular patient the parents were allowed to visit any time they wished, and they were given an excuse from their work for this purpose. Before this patient is discharged, the doctor will give the parents an explanation for their daughter's difficulty and tell them that since the girl is coquettish and enjoys reading, they should support these positive character traits.

The patient was still speaking in a whisper and her current therapy was directed to this problem, with the doctor persuading her that she must prepare herself for school, and suggesting that she would be able to speak by September, then only 7 weeks away.

These Russian child psychiatrists used play therapy only with pre-school children. Since this patient was of school age, no toys were used during the interviews. They felt that they had made an exception to hospital policy in giving her a doll, which she kept with her all the time.

The patient was presented to our group and was asked to recite her poem for us. She did so with a raspy voice. After this her therapist asked her about 15 questions, each one of which seemed to have a built-in answer. For example, she was asked if she was a good girl, and her answer was "Da."

"Do you like animals?" "Da."

"Do you like the monkey?" "Da."

"Did you speak when you were at home, *i.e.* before you came to the hospital?" "Nyet."

"Are you feeling better now than you

did when you first came here ?" "Da."

"Which one of your sisters do you not like ?" At this question the patient hung her head. The only spontaneous thing the child said was, "I want to go home."

The diagnosis for the little girl was "nervous in the form of mutism."

When I asked about the child's apparent immaturity, they dismissed this as being clinically insignificant and said that she was immature because she was the youngest of 6 children and was used to being treated like a baby.

We also discussed the Russians' system of outpatient psychiatric clinics. The Moscovites have a wide network of general medical clinics in all sections of the city, and each of these clinics has a child psychiatrist assigned to it. In addition, there is a special clinic for psychiatric patients only, with speech therapists and child psychiatrists on the staff. Finally, there is a central psychiatric clinic staffed by professors from the medical school, and the more difficult cases are sent there.

The outpatient therapeutic program in these clinics includes the use of tranquilizers, vitamins, electro-sleep, and speech therapy. When I asked the doctors about psychotherapy, they said that sometimes they educate the parents by telling them what to do, and they will also have a conversation with the child, trying to get him interested in some new activities. They frequently refer children to the Pioneer's Clubs and also work very closely with the schools in trying to get them to work out special programs for the child.

When I asked about delinquency, the answer was, "Nyet." They state that they do not have any young delinquents. The 16 to 18-year-olds are grouped with the adult offenders who are seen by prison psychiatrists rather than child psychiatrists. They hold that delinquency in a child under 16 years of age is very rare, perhaps one case in 4 or 5 years.

I asked about school phobia, which they said is also very rare. When it occurs, they felt it was because the child was too young to go to school or too infantile, so they let him stay at home another year. They did not see school phobias at the time children enter secondary school ; and I suspect the reason for this is that the children with such difficulties enter the labor market.

They also deny any problem with bright children who do not learn. In part, the theme here was that everybody in Russia wants to learn, but it also developed that they used no standardized psychological tests in Russia : consequently, children are not identified by this means. The other way in which under-achieving students might be identified, through the school teacher, would also not necessarily bring them to light since such problems are considered to be due to inadequate teaching.

Further inquiry about the matter of psychological tests revealed that they do not use the "Binet" or any modification of it, nor do they use the Rorschach. They do use association tests, and they speak of intelligence tests in which they will give a child a task to do. However, these tests are not standardized and apparently they depend, to a large extent, upon the subjective impressions of the examiner.

They do not have any social workers on the team, "since all citizens of the Soviet Union are considered social workers." Nor do they have any foster home program, and dependent children are placed in institutions if they are not adopted. The child psychiatrists have no part in these programs.

I had heard that there was some private practice of psychiatry in Russia, and when I asked about this they laughed and said, "Nyet," in such a spontaneous way that it suggested they really did not believe such existed. They said that private practice existed only in the field of dentistry. John Gunther in *Inside Russia Today* and Thomas Hammond in the September, 1959 *National Geographic Magazine* both state that there is some private practice of medicine in Russia.

These Russian psychiatrists' objection to psychoanalysis was that they did not consider it a scientific theory. When I asked what they meant by this, they said that they feel that Freud considers sex only, and that they are not able to see this sexual material in their patients. My feeling was that they couldn't very well expect to see it since they do most of the talking and rarely give

the patient an opportunity to talk, let alone free-associate.

My visit with these 7 Russian child psychiatrists, two of whom were professors in charge of post-graduate training in child psychiatry in Moscow, was very pleasant. All were friendly and seemed to be genuinely interested in children. They eagerly participated in our talks and seemed to have a strong desire to share experiences. They asked questions of me for almost an hour, and listened to my answers with interest. When I told them that we would be more inclined to consider the nature of the psychopathology underlying the little girl's symptoms; would feel that the sister's attack was a precipitating event, rather than the causative factor in her neurosis, and that we would probably evaluate her immaturity in terms of the mother-child relationship, they listened as if they had heard similar formulations before, but were not impressed.

One final word to emphasize a situation in Russia much different from any that I know of in this country. In talking with a Georgian high school teacher of English about some of the academic and behavior problems he runs into, I learned that they quite consistently deal with these problems along the following pattern. (Incidentally, such problems are not referred to child psychiatrists. They are dealt with by the teachers and school administrators.) Everyone in Russia belongs to a number of different organizations; the workers in the factory have an organization, the students in each grade have an organization, the teachers have an organization, the parents of the school children have an organization, and so on. When a student is having a problem, this will be brought to the attention of as many as 4 or 5 organizations to which the child and his parents belong. Each organization will begin formulating a program to help him out. Frequently when children are having trouble at school, the workers' organization at father's factory will be informed and will attempt to work out some form of environmental manipulation for the child. His problem will also be discussed at the organization meeting of his schoolmates, who will try to formulate some helpful program. The parents at the school "P.T.A." organization will be informed, as will the teachers' organization, and so on, so that the child will have a number of groups all expressing concern about him and trying to be helpful in various ways. They maintain that the results of this usage are very encouraging. When a student has difficulty, everybody gets together and tries to find out what he likes to do (athletics, chess, drama, dancing, music, handicraft) to help him foster this interest.

If a child is doing poorly in school, his teacher is the one who is held responsible. The teacher is also considered responsible if the child is truant.

The teacher also sees the parents of problem children every week, and sees the parents of all the children in his classes at least once each month. Many of these contacts are home visits, and the teacher I talked with, said that he knew the parents of all his students quite well.

I appreciated the opportunity to exchange views with these Russian child psychiatrists. The warm climate of our professional interchange was both pleasant and encouraging.

EXTRA-SCIENTIFIC INFLUENCES IN THE HISTORY OF CHILDHOOD PSYCHOPATHOLOGY[1]

M. B. MACMILLAN, B.Sc.[2]

INTRODUCTION

Examination of 19th century literature relating to psychosis in childhood shows that the psychotic child was an object of study in the first part of the century, and that a substantial body of knowledge on the subject was accumulated in that period. Toward the latter part of the century, not only did the level of knowledge decline, but the psychotic child even seems to have ceased to have been an object of study.

The purpose of this paper is to substantiate the above assertion and to examine the scientific and extra-scientific factors giving rise to it.

EARLY LITERATURE ON CHILDHOOD PSYCHOSIS

Spectacular cases of insanity in children have been recorded, of course, since the earliest times. Greding, for example, writing in the late 18th century, cited the following:

> A woman, about forty years old, of a full and plethoric habit of body, who constantly laughed and did the strangest things . . . was, on the 20th. January, 1763, brought to bed . . . of a male child who was raving mad. When he was brought to our work house, which was on the 24th., he possessed so much strength that four women could at times with difficulty restrain him. These paroxysms either ended in an uncontrollable fit of laughter, for which no evident reason could be observed, or else he tore in anger everything near him . . . We durst not allow him to be alone, otherwise he would get on the benches and tables, and even attempt to climb up the walls. Afterwards, however, when he began to have teeth he died (quoted by Maudsley, 10, p. 258.)

In general, however, the recognition of insanity in children seems to have been delayed until the various early training centres for mentally defective children were established. Seguin, in the 1866 revision of his *Idiocy and its Treatment by the Physiological Method*, recognises "incipient insanity" as a cause and complication of idiocy proper. He probably so recognised it in the early edition, which is, unfortunately, unavailable to the writer. For 10 years between 1845 and 1860, Griesinger was the director of "the idiot asylum of Mariaberg"(6). He observed that there, and "in the several institutions recently erected for the reception of children with weak intellect, there are generally found more or less special cases of mental disease." Amplifying this observation, he described psychoses in intellectually normal children, as well as psychotic reactions in defective children.

Griesinger believed that both maniacal and melancholic forms of insanity were found in children; that is, the recognised range of adult forms was to be found also in children. What was covered by his terms may be clearer from these quotations, firstly, on maniacal conditions:

> Sometimes they appear as persistent or even habitual moderate irritability of character: The child is passionately obstinate, quarrelsome, malignant and even inclined to immorality . . . Sometimes it is a state also persistent, but more intense: there is greater restlessness, a constant aimless roving, confusion of the intelligence, perversion of the emotions . . . which . . . sometimes passes into profound mental weakness. It is impossible definitely to distinguish this from the versatile form of infantile dementia:[3] these children cannot keep quiet even for a moment; they talk incessantly and incoherently, pay no attention, constantly wander about, laugh, cry, *etc.* . . . Sometimes there are . . . attacks of really developed mania (*i.e.*, corresponding to the adult forms).

Among others who would have been included in the category of "maniacal conditions" would be the acting out aggressive

[1] Based upon a paper read at the Annual Conference of the British Psychological Society, Australian Branch, at the University of Melbourne, August, 1959.

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[3] By this term Griesinger meant one type of defective child (*c.f. op. cit.*, p. 376).

child, the schizophrenic child and the hyper-active child.

Griesinger believed the melancholic forms to be less common than the maniacal. Under this term he included hypochondria, generalised anxiety, suicidal states, ideas of possession by demons. Delusional states he considered to be absent in childhood as "no persistent ego is as yet formed in which there could occur a lasting radical change; the mobility of this age does not allow insane ideas to become persistent . . ."

The factors causing psychosis in childhood were enumerated by him as follows: cerebral irritability either due to heredity or to injudicious treatment (intimidation, ill-treatment of mind, intellectual overexertion, dissipation), organic disease processes, sympathetic stimulation of the brain from the genital organs as a consequence of masturbation on the approach of puberty, *etc.* It can be seen that he gives at least equal weight to psychological factors (intimidation, *etc.*) as to physical. This is in line with his more general statement of the causes of insanity. These he divides into the predisposing and immediate, saying of the psychical, that they

> . . . are, in our opinion, the most frequent and fertile causes of insanity, as well in regard to preparation as especially and principally the immediate excitation of the disease.

Maudsley, who, after Griesinger, appears to have been the main authoritative writer to have considered the psychotic child, attempted to relate the type of psychosis to the level of development reached by the child. Thus he describes the sensory-motor insanities (the epilepsies, choreic movements associated with hallucinations) and the delusional (related to the development of stable ideas). This scheme is not followed consistently for it is soon abandoned for the adult classification(10). The notion of instinctive insanity is introduced and developed to a greater degree than in Griesinger. Coupled with this, the concept of instinctive degeneration is used to describe the various diseases. In contrast with Griesinger, who at least mentions the possibility of using Seguin's educational methods for treating the maniacal conditions (6), Maudsley says nothing regarding treatment. Maudsley's discussion of the subject, ambitious as it is, is at a lower level than that of Griesinger. And it is certainly true that those writing after Maudsley reached nothing like the same level.

Ireland's chapter, "On Insanity in Children and Insane Idiots and Imbeciles," of 1877(7) mentions both Maudsley and Griesinger by way of introduction but is little more than a collection of case descriptions. Twenty-three years later his revision of this chapter shows changes only in terminology and systematisation(8). Other writers, like Bucknill and Tuke(3) and Sankey(11) do not mention the subject. With a few notable exceptions, the *Journal of Mental Science* published or reviewed little on childhood psychosis between 1870 and 1900. Beach's paper(2) is one such exception, but, by the time it was published it was as late as 1898. It is of interest that Shuttleworth, in the discussion on this paper, pleaded for sharper distinctions to be made between mental defect and psychotic conditions.

In fact it could almost be said that, after Maudsley, it was not until Sancte de Sanctis described the entity of dementia praecoxissima in 1905-1908(4) was the psychotic child again to become an object of study.

Having now substantiated the assertion that the level of knowledge concerning the psychotic child declined steadily between the early and latter part of the last century we now turn to an examination of the possible reasons for this peculiarity in the development of psycho-pathological studies. The factors determining this development are considered under two headings: scientific influences and extra-scientific influences. Scientific influences are defined as those influences arising solely from the scientific nature of the problem, such as the effects of method of study and general level of scientific knowledge. By definition, extra-scientific influences are those arising outside of these. They include such influences as social attitudes and political views.

SCIENTIFIC INFLUENCES

The main set of scientific influences are to be found in the early history of mental defect. From the standpoint of our present

day knowledge it is clear that the classification of mental defect should be based upon 3 related, but independent, criteria : the educational level (*e.g.* educable v. trainable), the pathological type (*e.g.* mongolism, microcephaly), and the assumed aetiology (*e.g.* heredity, foetal damage, birth injury). Tredgold seems to have been the first writer to have recognised the need for this threefold method of classification. Prior to him, there is obvious confusion on this point(1).

There is also a certain amount of confusion in the recognition given to distinct pathological types. While the micro-macrocephalic distinction was made early (for example by Seguin), and cretinism constituted an entity from at least the time of Guggenbühl, few other distinctions seem to have been made (at least in such a way as to be understandable to the modern observer). Such a distinct type as mongolism, for example, was not recognised until the late 1860's and not generally accepted until some time later(7). Additionally there were a number of instances where one type of mental defect was considered to be the only one(6).

The immediate reason for this confusion and for the failure to recognise distinct pathological types appears to have been the emphasis given to the educational criterion as opposed to the other two. Since the problem is a three-fold one, to emphasise any one criterion of classification means, in the early stage of a science, not to do justice to the other criteria.

Since this early emphasis upon the educational aspect of the problem precluded pathological and aetiological consideration and since the psychotic child resembled the truly defective child in being intellectually handicapped, it is not surprising that the psychotic child did not become, in this early period, an object of study in his own right. Seguin's unconcern with the problem of the psychotic child is explicable on this view.

Griesinger's relatively advanced views on the subject require additional explanation, however. In the writer's opinion, these follow from the fact that, not being primarily an educationalist and not being personally concerned with the training of the defective child, Griesinger could pay more attention to the behaviour of the children. This allowed him to develop a classification based upon behavioural types and direct attention towards aetiology. Both of these tendencies enabled him to identify the psychotic child.

The gradual loss of emphasis upon the educational aspect of the problem, a precondition for the development of pathological and aetiological studies, probably also resulted in part from the rather crude approach to learning. As the training techniques became exhausted a changed emphasis would follow.

Extra-scientific influences played a part and, as will be seen, determined the peculiarities of the pathological approach which did develop as the educational emphasis waned. They were also responsible for the fact that the initial emphasis was on the educational aspect. These influences are now examined.

EXTRA-SCIENTIFIC INFLUENCES

The extra-scientific influences are of several kinds : directly socio-political, those derived from political influences and those derived from social attitudes.

Direct and derived socio-political influences. It is of considerable interest that Seguin, the main figure in the early history of mental defect, was interested in the problem of the education of the defective child for direct socio-political reasons. Himself a Christian Socialist of the St. Simon school, he saw in his extension of Itard's famous effort to educate the savage of Averyron (itself based upon the even more radical socio-political views of Rousseau and Condillac), an attempt at

> . . . a social application of the principles of the gospel ; for the most rapid elevation of the lowest and poorest by all means and institutions ; mostly by free education(12).

For Seguin, the task of educating the idiot was part of the wider movement for the abolition of social classes and the establishment of a just society. He eventually came to feel that his efforts, frustrated in the France of that time, had come to fruition in the stronghold of democratic ideals, the expanding economy of America.

Quite apart from the direct testimony of Itard and Seguin, there is some evidence that the acceptance of the idea of educating the defective child was conditioned by the more general acceptance of the policy of mass education. As Seguin put it,

> . . . it is not enough for an idea to be ripe in the mind of a thinker . . . the social medium in which it falls must be prepared for it as well; otherwise no production ensues from their contact . . . generally the ground rejects the seeds which it cannot germinate, and they are carried . . . to a more genial soil.

Seguin himself claims a relation between the acceptance of the idea of mass education and of the education of the defective. The discussion of Barr(1) and Ireland(8) tends to support this view. At least it is true that, even at the end of the last century, France lagged behind other countries in the provision of facilities for the education of both defective and normal children.

In so far as the movement for mass education had a socio-political basis, this current of influence must be classed alongside the more direct influence of radical thought.

Changes within the educational movement itself seem also to have played a part. It seems reasonable to suppose that as the early optimism about the possibilities of education of defective children gave way to a more realistic (and even pessimistic!) appraisal, some of the earlier impetus would be lost. In France the non-achievement of the goal of mass education seems to have sapped the energies of those concerned with the problem(1). These developments would also have assisted in changing the emphasis to pathological and aetiological matters.

The argument in this section, that extra scientific attitudes determined the efforts of the early educationalists, may be inferentially supported by the history of Zilboorg and Henry(14). Their delineation of what they call the first and second psychiatric revolutions rests upon changes in social and political thinking. More specifically they evaluate Pinel's work against the background of the French revolution, which . . . awakened everywhere not only the sense of the individual's social responsibility but particularly the sense of the community's responsibility towards its members.

The same feelings of responsibility were clearly held by most of the early workers in the field of mental defect.

It is both unfortunate and curious that Zilboorg and Henry chose to exclude completely the history of the study of mental defect. Unfortunate because some of the points made here may have been otherwise directly substantiated. Curious, because the topic of mental defect was given so much attention by the early writers. Seguin and Guggenbühl, for example, are not mentioned at all. That period of Griesinger's work concerned with mental defect and childhood psychosis (between 1845 and 1861) is simply omitted from their biographical notes. After tracing Griesinger's history up to the publication of *Pathologie und Therapie der psychischen Krankheiten* in 1845, they blandly state

> Griesinger did not resume his contact with clinical psychiatry until 1866 when he became chief of the division of mental diseases at the Charite in Berlin.

Yet, as has already been noted, it was precisely during this period that Griesinger had been in charge of "the idiot asylum" at Mariaberg. Perhaps Zilboorg and Henry's neglect of this fact together with the whole field of mental defect springs from their tendency to evaluate the history of psychiatry in terms of the acceptance or non-acceptance of psychodynamic formulations by the earlier psychiatrists. Mental defect is definitely an area where such formulations are of secondary importance to that of the relation between mind and brain.

Influences from changed social attitudes.

Darwin's publication of *The Origin of Species* in 1859 did more than revolutionise the study of biology. Considered either as a cause, or as the consequence of other intellectual developments, it acted as the focal point for the spread of the materialist mode of thought. Not long after it's publication Langdon-Down's ethnological classification was made. Although it soon became only an historical curiosity, it is

of interest in that it demonstrates a direct influence of the materialist-evolutionary thinking of the time upon the distinction of pathological entities of mental defect.

Other, more indirect influences are also evident. The whole basis for the distinction of pathological types became a physical one. Griesinger, whose observations predated Darwin's work, seems for this reason, as much as for those discussed earlier, to pay some attention to behavioural characteristics of defective children. Thus, after discussing the anomalies of perception, of the "desires," of movements, *etc.*(6), he distinguishes "two fundamental forms" of idiocy :

> the apathetic (dull, torpid) and the excited (versatile, agitated). The profound idiots of the first category have frequently an awkward, clumsy and disproportioned body, and repulsive old-looking features ; the dullness of their movements, their passiveness—their stupid, monotonous unexcitable demeanour—cause them in many cases to appear as if they were in a state resembling sleep . . . Those of the second category are really (? rarely-M.B.M.) much deformed, but generally remain far behind in their years . . . they are restless in their movements, quick, irritable, rapidly change their impressions . . . It is often astonishing, when we see the happy expression and apparent activity of these children, to find that they are utterly incapable of speech and void of understanding. In many cases the behaviour is often so excitable . . . that it actually appears to pass into mania.

Among later writers, Kerlin (cited in 1) appears to have been the only one who developed this line of approach. Not until Earl revived it in 1934 were the behavioural characteristics themselves made the basis of a typology(5). At the present time interest in the matter has again revived ; if one can judge from the titles of papers presented to the 1958 Annual conference of the British Psychological Society in Birmingham.

Another potent influence appears to have come from a changed social attitude (of unknown origin) to children. This may be illustrated by a comparison of the views of Maudsley and Griesinger. Maudsley opens his chapter on "The Insanity of Early Life" with the following words :

> How unnatural ! is an exclamation of pained surprise which some of the more striking instances of insanity in younger children are apt to provoke. However, to call a thing unnatural is not to take it out of the domain of natural law . . . Anomalies, when rightly studied yield rare instruction . . . For this reason it will not be amiss to occupy a separate chapter with a consideration of the abnormal phenomena of mental derangement in children(10).

To be contrasted with this are Griesinger's straightforward opening remarks :

> During childhood (before puberty) insanity is not frequent, but almost all forms of it occur. Those most generally observed are the various kinds of mental weakness . . . next in order . . . come the maniacal conditions . . .

That Maudsley himself did not think it unnatural that children should develop psychoses is implicit in the paragraph quoted. It is more obvious, perhaps, in the following remarks :

> To talk about the purity and innocence of a child's mind is a part of that poetical idealism and willing hypocrisy by which men ignore realities . . . ; in so far as purity exists it testifies to the absence of mind ; the impulses which actually move the child are the selfish impulses of passion. It were as warrantable to get enthusiastic about the purity and innocence of a dog's mind. "A boy," says Plato, "is the most vicious of wild beasts," or, as someone else has put it "a boy is better unborn than untaught"(10).

Anyone with views like these would surely not be thrown off balance by the development of psychosis in a child. Is not Maudsley actually addressing his readers, the intelligent laymen and the psychiatrists of the day, in the expectation that they will be surprised ? If so, the apology reflects a change in reader attitude from Griesinger's time. Some support for this argument may be derived from comparisons between the 2nd. and 3rd. editions of Maudsley's textbook(9, 10). His earlier discussion, of 1868, does not include the topic of the "unnaturalness" of psychosis in childhood. Neither is any comparison drawn between the child and the dog in the earlier passage relating to "purity" of mind. One cannot help wondering if these

remarks reflect Maudsley's protest against the Victorian attitude of sentimentality toward children; an attitude leading to a denial of the possibility of childhood psychosis.

CONCLUSION

Hence, extra-scientific influences determined early interest in the problem of the defective child. Either of a direct or indirect socio-political character they created a field of study which included the psychotic child. However, because their influence was an educational one, the psychotic child, along with other pathological entities, was not distinguished as such. Only after the educational emphasis had changed were the conditions created for the emergence of the psychotic child as an object of study. But this did not happen when the change took place. The emergence of the psychotic child as an object of study was further delayed by the physical emphasis of those interested in establishing pathological typologies. As has been shown, this was dependent upon extra-scientific factors, notably the strength of the mechanist materialist mode of thought. The differences between the views of Griesinger and Maudsley seem to have been determined by the fact that the former's outlook was constricted neither by his mode of thinking nor by the changed social attitude to children.

SUMMARY

This paper attempts to account for the peculiarities in the development of 19th century knowledge of the psychotic child in terms of both scientific and extra-scientific factors.

It is argued that extra-scientific factors determined the early interest in the defective child with whom the psychotic child was first classed. A combination of both scientific and extra-scientific factors is claimed to account for the almost complete lack of interest in the problem in the second half of the century. An attempt is also made to account for the differences between the views of Griesinger and Maudsley.

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PRECISION IN PSYCHOANALYSIS

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The purpose of this paper is: (a) to show that precise definition of terms is necessary if psychoanalysis is to become more scientific and useful, and (b) to demonstrate two contrascientific trends within classical psychoanalysis resulting from its lack of precision: a refusal to face facts, and a tendency to retreat to a therapeutically-nihilistic "elite" status(1). These retreating trends tend to evoke hopeless abandonment of the interpersonal search for the cause and cure of mental illness, thereby leaving the field to chemists, geneticists and physiologists whose tools, though refined, appear unsuited for the analysis of human feelings and mental illness.

We shall first attempt to define the word "scientific," and to indicate the role of precision in scientific work. Second, we shall examine a recent classical psychoanalytic conceptualization to demonstrate its lack of precision and its unverifiability. Third, we shall try to show that a scientific reformulation of this conceptualization may make experimental and clinical verification possible. Fourth, we shall examine Freud's metaphorical method to show that it lacked precision and frequently confused reiterated hypotheses for proved facts. Finally, we shall examine Freud's unscientific justification of his opposition to precise definitions, and shall seek the consequences in American classical psychoanalysis today.

WHAT DOES "SCIENTIFIC" MEAN?

The Merriam-Webster Unabridged Dictionary defines "scientific" as "conducted . . . strictly according to the principles and practice . . . of exact science, especially as designed to establish incontestably sound conclusions and generalizations by absolute accuracy of investigation."

The phrase "incontestably sound" includes the basic concept of verifiability. Descartes' scientific concept of how the brain worked, as the neurophysiologist H. S. Magoun notes(2), "was so clearly put as to possess the danger of permitting easy determination of its truth."

If "scientific" means "conducted according to the principles and practice . . . of exact science," we can examine the practice and principles of an exact science for guidance. Nobel Laureate Robert A. Millikan(3), writes:

> The first principle of the physicist, when he uncovers a new phenomenon, is to determine *what* he is to measure; the next is to devise *means* to measure it(3a) . . . All scientific investigations which have led to real progress have begun . . . by the treatment of simple and specific problems with quantitative exactness, not by making deductions from general philosophical schemes or *a priori* principles (3b).

One of the cardinal principles of scientific work, therefore, is precision, both in definitions and in measurements.

A RECENT VAGUE PSYCHOANALYTIC CONCEPTUALIZATION

Percival Bailey's recent Academic Lecture(4) criticizing classical psychoanalysis drew an almost definitive reply from Ostow(1), one of the ablest of the classical psychoanalysts. Let us apply the principles of scientific methodology which Millikan has just described to one of Ostow's key statements.

As a hypothesis, Ostow offers the psychoanalytic proposition that *"every man has a tendency to enjoy a physical, sexual relationship with his mother."* Verification of this statement requires, as Millikan indicates, determining *what* is to be measured, and then determining *how* it is to be measured.

As is unfortunately so frequent in psychoanalytic statements, many words in Ostow's hypothesis have rather vague meanings. His use of the word "every" would mean the proposition disproved if one man on earth lacked this tendency. Does the word "man" mean an adult male, or does it mean *all* human males, or does

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it mean all young human males, *i.e.*, boys? I believe Ostow is really referring to boys rather than to men.

But the key word in Ostow's hypothesis is "tendency"; what does it mean? The total proposition cannot be scientifically examined until we know *what* this "tendency" is which we are measuring and against what yardstick.

The word "tendency" itself is an example of the fuzziness of definition so frequently seen in psychoanalytic writings. A "tendency" has a self-initiated efferent quality quite similiar to that present in the psychoanalytic concepts of wishes, drives, impulses and instincts. But all these efferent concepts have two separate aspects: that which is either innately or experientially responsive to stimuli, and that which is mystically self-propelled, as were Freud's life and death instincts. Failure to distinguish between the responsive and self-initiated aspects makes Ostow's meaning rather unclear.

For the sake of discussion, let us assume we know what Ostow's "tendency" means. How do we verify its existence?

We are usually told that the hypothesis is validly confirmed by patients' productions in psychoanalytic treatment. But Grinker(5) denies that such confirmation is valid. He says:

> Using the tools of psychoanalysis, (psychoanalysts) find what they search for and little else. . . . The patient is the psychoanalyst's biased collaborator. Each interpretation may be a hypothesis . . . but there are no alternatives and little possibility that the patient-collaborator will refute it, although theoretically much is made of the patient's behavior as an index of correctness or refutation of interpretations. We have demonstrated in our experiments . . . (that) the patient-subject interprets almost everything that the psychiatrist states as having therapeutic meaning. The patient is not an unbiased scientific colleague.

Verification of a psychoanalyst's hypothesis by his patients' responses is, therefore, not a scientifically valid, independent confirmation of the hypothesis, particularly when the analyst determines whether the student-patient progresses in his course of psychoanalytic training. Instead, it may well be part of a closed philosophical system.

What other means are there of direct verification of hypotheses such as Ostow's? Is selecting(4a) "from a mass of data of observation those items which support (one's) thesis" worth while? Such a selection is valid in the formulation of hypotheses, but not valid in proving them.

Isaac Newton(6) pointed to the lack of validity of "proofs" arrived at by such selection of data. He wrote:

> The best and safest method of philosophizing certainly seems to be, first, to inquire diligently into the properties of things, and to establish these properties by experiments.

Psychoanalysis has had relatively few such experiments. Newton continues:

> Then, [one should] proceed more slowly to hypotheses for the explanation of them. For *hypotheses* ought to be used only in explaining the properties of things, and ought not to be assumed for determining them, *except where they are able to furnish experiments.* For if from the possibility of *hypotheses* alone, anyone makes a conjecture concerning the true nature of things, I do not see by what means it is possible to determine certainty in any science, since it is always possible to devise any number of hypotheses, which will seem to overcome new difficulties.

Hence, selection of data to fit a hypothesis by no means proves it; confirming a hypothesis is more important than, and should precede, elaborations upon it.

Is the "general opinion" of the correctness of psychoanalytic concepts proof of its validity? If this were so, the earth suddenly became spheroidal in 1492, after having previously been flat, and no one needed psychoanalytic aid before Freud appeared. "General opinion" therefore lacks validity as scientific proof. Consequently we must conclude that there are no direct proofs for the validity of Ostow's imprecisely formulated psychoanalytic hypothesis.

The only indirect proof for the classical psychoanalytic hypotheses lies in the value of the procedure in helping patients. Yet Teuber(7), discussing the Cambridge-Somerville experiment, points out that "the burden of proof is on anyone who claims

specific results for a given form of therapy." But when the American Psychoanalytic Association examined its members' treatment results, it could not prove the value of the classical psychoanalytic treatment. Weinstock, chairman of the survey committee, stated(8), "It is not that the figures can be used to prove analytic therapy effective or ineffective." Hence indirect therapeutic proof of the classical psychoanalytic hypotheses is also lacking.

Consequently, there is no proof, direct or indirect, for Ostow's imprecisely formulated psychoanalytic Oedipal hypothesis.

A VERIFIABLE REFORMULATION OF THE OEDIPAL HYPOTHESIS

Is there sexual attraction (a more precise formulation than "a tendency to enjoy a sexual relationship") between a boy and his mother? I believe there is, because there are apparently inborn sexual responses in all of us to members of the opposite sex which may well be mediated outside of conscious awareness, and at least partly through the sense of smell(9). Since the mother is the female with whom the boy has most contact, his inborn sexual responses will probably be directed mostly toward her, but there may well be a corresponding unconscious sexual response on her part toward him as well. These concepts, unlike classical formulations such as Ostow's, can perhaps be experimentally tested and quantitatively measured. But until such testing confirms them, they must be regarded as hypotheses only, no matter how many times our patients may confirm them, unless, perhaps, it can be clearly shown that using them is statistically helpful in accomplishing cure.

AN EXAMPLE OF FREUD'S METAPHORICAL VAGUENESS

Let us now examine an example of Freud's metaphorical method, to see the lack of precision characterizing much of his work. That this led to later mysticism, with life and death instincts in eternal unverifiable conflict, is well known. We shall, however, take an example from the more scientific early part of his career.

In his "Interpretation of Dreams," he discusses the behavior of a hungry infant, and states(10), "nothing prevents us from assuming that there was a primitive state of the psychical apparatus . . . in which wishing ended in hallucinating." In this connection, let us recall Newton's statement(6) that "it is always possible to devise any number of hypotheses which will *seem* to overcome new difficulties."

Freud maintains, essentially, that since the baby has been fed before, when he is again hungry, the memory image of the previous feeding might be experienced as a hallucination. But there is quantitatively a far cry between the memory trace of a previous feeding in a two-day-old baby, and the relatively adult quality of a hallucination. An hallucination of milk involves the anticipatory differentiation of milk from non-milk, something utterly beyond the capacity of a two-day-old infant. Moreover, while perhaps(10) "nothing prevents us from assuming" this hallucination, scientific method demands, as Newton(6) points out, that such hypotheses "ought *not* to be assumed for determining (the properties of things), *except where they are able to furnish experiments.*" In the 59 years since Freud's assumption of infantile hallucinations, what experiments have been made to prove or disprove them? Yet this and many other unprovable assumptions continue to be accepted in psychoanalytic thinking because one great man postulated them.

But here, and elsewhere as well, Freud's lack of precision led him down an unverifiable and therefore unscientific path. Was this imprecision accidental? Perhaps it was at first, but it was later explicitly justified.

We have seen how Millikan insisted that hypotheses cannot be verified until *after* they have been rigorously defined, and that hypotheses cannot themselves be defined until their fundamental terms and basic concepts have previously been rigorously defined. But psychoanalysis consciously and explicitly declines to define its terms rigorously; it is as if Freud's statements are, *ipso facto*, sometimes exempted from the scientific requirement for objective verification. In reality, however, it is

a long way from "nothing prevents us from assuming" to proven fact.

FREUD'S JUSTIFICATION FOR "ELASTIC" DEFINITIONS

The question of "clear and sharply defined basal concepts" is discussed in Freud's 1915 paper, "Instincts and Their Vicissitudes"(11). His concept of scientific method differs quite sharply from that of the exact scientists already quoted. It also differs sharply from Osler(12), who wrote that "the leaven of science gives to men habits of mental accuracy . . . which enlarge the mental vision."

In his 1915 paper, Freud correctly points out that concepts are changed as a science progresses. Continued investigation reveals conceptual imperfections, and the concepts and definitions are therefore changed accordingly. Because, at the beginning of a scientific investigation, we do not know its final concepts with *absolute* accuracy, Freud incorrectly denied the necessity of precise definition of the *relatively* accurate conceptual tools with which the investigation begins.

When we deal with precisely defined concepts such as Newton's or Descartes', we can fairly easily determine the accuracies and inaccuracies within them. Such determinations result in more precise formulations, which are then subjected to the same evaluative process, leading to still greater precision.

When, however, we have no firm definitions with which to work, we find ourselves without a valid starting point. We are consequently attempting to dissect warm air with empty hands. The fact that concepts and definitions become altered as the result of investigation is very different from the idea that *working* definitions, like scissors, should be elastic.

Hence Freud's statement(11) that "the progress of science demands a certain elasticity even in . . . definitions" is unscientific. Working definitions are points of reference, but not rubber bands; they can be and are changed, but they are not elastic. They become *different* definitions after their alteration, rather than merely being somewhat stretched. A sexually altered dog is sexually quite different from what it used to be; there is no question of elasticity whatso ever. Just as an oak is quite different from the acorn from which it has grown, so is an hallucination quite different from an infantile memory trace.

But, it might be said, psychoanalysis differs from physics and mathematics inasmuch as it deals with the unconscious, with feelings and with instincts. Ernst Mach(13) categorically rejected abdication of scientific method to the "instinctive." He wrote:

> Instinctive knowledge is very frequently the starting-point of investigations. . . . This by no means compels us, however, to create a new mysticism out of the instinctive in science and to regard this factor as infallible. That it is not infallible, we very easily discover. . . . The instinctive is just as fallible as the distinctly conscious.

Freud failed to separate meticulously the mystical, self-initiatory aspects in his concept of instinct from its scientific responsive aspects. This failure, continued by some of his followers, has helped lead to the pessimistic religious trend which has pervaded much of classical psychoanalysis for so long. Indeed, the psychologist Joseph Lyons(14) recently noted that

> If there is one all-pervading faith that binds twientieth century western man, it may be found in his uncritical acceptance of the value of psychotherapy. If there is a universal answer offered in these times for the anxiety that is supposed to be the mark of the age, it lies in the role of the patient in psychotherapy. It is our new religion, arising out of and efficiently tailored to the moral crisis of the day.

THE RESULTS OF IMPRECISION IN CLASSICAL PSYCHOANALYSIS TODAY

Freud's refusal, continued by his followers, to define terms meticulously has led, in part, to the rather poor estimation other scientists hold of psychoanalysis. James R. Newman(15), author of *The World of Mathematics*, recently reviewed a new psychological and psychoanalytic dictionary. Referring to these fields, he wrote:

> A discipline cannot live without words, but words can corrupt and destroy it. This explains the importance of good science dictionaries, which are as much works of criticism as they are guides to usage. No subjects are in greater need of such services than psychology

and psychoanalysis. The vocabularies of both these wildly flourishing branches of study are plagued by amateurishness, pretentiousness and a general professional weakness for fancy terms. As Goethe wrote in Faust, "When ideas fail, words come in very handy."

This is one example of an exact scientist's view of psychoanalysis today.

Percival Bailey(4a), the distinguished neurosurgeon happily turned psychiatrist, wrote :

I know that there are attempts to prove that psychoanalysis is a science. They do not convince me and have convinced very few objective observers(4b). Even Freud(4c) admitted that it is only a sort of post-dictive science, lacking in power of synthesis and prediction. Science cannot be built on the insights of visionaries or on the mutual titillation of interdisciplinary minds at Palo Alto, or elsewhere, Science can be built only by the cautious, laborious verification, step by step, of one's hypotheses, establishing each one solidly before passing on to the next. As Jones says (4d), Freud had no patience with such a method.

This is another example of an exact scientist's view of psychoanalysis today.

TWO UNFORTUNATE PSYCHOANALYTIC RESPONSES

Two important contra-scientific trends can be seen within classical psychoanalysis in response to its general scientific vagueness and to its specific failure to prove its therapeutic effectiveness. The first trend declines to reveal the data about its lack of therapeutic effectiveness, and seeks to rationalize away this anti-scientific suppression of data. The second trend maintains that only a psychoanalytic "elite" are capable of meaningfully evaluating both themselves and their results.

The first trend is exemplified in Weinstock's explanation of the American Psychoanalytic's decision not to publish its survey results. "The material on which either opinion is based (whether or not psychoanalysis is therapeutically effective) is inadequately established, and controversial publicity on such material cannot be of benefit in any way." This fear of "controversial publicity" includes refusal to allow investigators who are not members of the American Psychoanalytic even to see the report unless they pledge in advance to keep the material "confidential." This is material which has already been circularized to the membership of the American Psychoanalytic, and which has already been described in detail in the *New York Herald Tribune*.

How scientific is this point of view?

Avoidance of "controversy" (*i.e.*, disagreement) and suppression of data because they might support the "wrong" side still occur in politics, but have not been in style in astronomy, for example, for about 350 years. At that time, Tycho Brahe spent 25 years making astronomical observations to destroy the Copernican heliocentric theory. His observations were, however, available to Kepler, who used them to prove the Copernican doctrine, and to place it on a firm foundation.

It would appear to me that the general public and the healing professions in particular would greatly benefit from the publication of the results of treatment at the hands of members of the American Psychoanalytic Association. The Bible says, "The truth shall make you free." It seems to me that the only people to whom "publicity on such material cannot be of benefit in any way"(8) would be individuals who, for some reason, may be afraid of what the truth will show. But science itself is more important than the reputation of any individual scientific worker, or of any particular group of workers.

The second unfortunate trend in classical psychoanalysis maintains that only the psychoanalytic "elite"(1) are capable of meaningfully evaluating both themselves and their results.

The analyzed are an elite . . . in the sense that they have had certain filters removed from their visual apparatus so that they can now see clearly what they previously could not see at all, or could see only with serious distortion(1).

While training analyses are often helpful, ascription of such crystal-clear thinking only to the products of "authentic" psychoanalysis (which Ostow contrasts with the "shoddy perversions and dilutions that usurp its name"), suggests a defensive device more than a statement of scientific fact.

For Ostow's statement to be completely

accurate and for all the filters to be removed, a perfect training analyst would be required. But no human being is perfect.

Indeed, there are data suggesting that the training analysis may even *add* visual filters not previously present. Edward Glover, as "authentic" an analyst as there is, writes(16),

> Training analysts' methods of analyzing candidates are influenced by their own character formations and peculiarities, and by the training they (themselves) have undergone. These peculiarities they, in their turn, are quite likely to transvey to their pupils.

I have known several analysts both before their training analyses began and since they have finished them. It seems to me, as an observant friend, that most of them are stiffer, less courageous and less human after their supposedly successful analyses than they were before. Some are members of the "authentic" American Psychoanalytic, and some are not. From my own small sample (hardly enough for a hypothesis, and certainly not for an assertion), those who have been "authentically" analyzed by training analysts of the "approved" New York institutes seem, in general, to be less warm, less spontaneous, less human and far more arrogant than those friends whose analyses were conducted under the aegis of one or another of "the shoddy perversions and dilutions that usurp the name" of psychoanalysis.

If classical psychoanalysts wish to make a secular religious cult of themselves, nobody can stop them, even if the consequences affect our entire society. It is also their privilege disdainfully to flee the epidemic of mental illness which Dr. Gunnar Gundersen, President of the American Medical Association, describes as sweeping our country.

But, as "authentic" Allen Wheelis writes (17), "Knowledgeable moderns put their backs to the couch, and in so doing may fail occasionally to put their shoulders to the wheel." Might not those who "authentically" worship Freud's great courage be more useful if they emulated it as well?

The more widespread the classical psychoanalytic retreat from American psychiatric realities, the more the classical analysts leave the field of investigation of mental illness to chemists, geneticists and physiologists, whose tools are not designed for the best available understanding of human feelings. There is some danger that this classical psychoanalysts' retreat from reality will tend toward the abandonment of perhaps the greatest contribution by Freud to psychiatry: his recognition that mental illness arises from distorted interpersonal relationships, beginning with the family of origin.

This retreat of some classical psychoanalysts also abandons the most potent tool there is in the field, a tool scientifically defined by Freud's genius, and used, although in part incorrectly, by him and his followers: the emotional interaction between patient and doctor. This retreat also abandons one of the most effective curative techniques yet devised in psychiatry: free association into the past to discover the "reminiscences" still plaguing patients.

All of these potent contributions to human welfare would be jettisoned should all of classical psychoanalysis withdraw to sulk in elite secrecy. It could then perhaps join other self-proclaimed aristocracies, such as Virginia Woolf's "aristocracy of sensibility." But is not retreat into such "elitism" an abdication of the physician's responsibility to the patients needing his aid?

I believe the science of interpersonal relationships which Freud founded can, when properly modified, lay open the causes and nature of functional mental illness. I believe that only psychoanalysis has forged the scientific tools able to overcome the effects of man's inhumanity to man, perhaps the prime cause of human fear and mental illness.

Fortunately, despite the negative trends mentioned above, psychoanalysis is far from dead. The incisive work of Ackerman (18) and others on intra-family interactions, the distinguished studies which Spitz (19) has made on insufficiently fondled children, Ferenczi's(20) demonstration that the analyst's warmth is a necessary condition for cure, and Fromm-Reichmann's (21) brilliant sensitivity with schizophrenics all encourage the hope that precise knowledge of the effects of interpersonal

warmth at the breast, in the home and role-appropriate warmth in the office can help us fulfill our task of preventing and curing mental illness. But for us to do so, we must also return to scientific precision, even in the presence of human warmth.

SUMMARY

Scientific precision has far too often been consciously excluded from classical psychoanalysis, because Freud rejected it. In consequence, classical psychoanalysis has assumed many of the trappings of a religion, and lost many of the essential characteristics of a science. Two anti-scientific trends in the field, defensive secrecy and arrogant "elitism," seem to have occurred in part as a result of perpetuation of this lack of precision.

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CREDIBILITY OF SUICIDE NOTES

JACOB TUCKMAN, PH.D.,[1] ROBERT J. KLEINER, PH.D.,[2] AND MARTHA LAVELL, M.S.S.[3]

In a study on affect manifest in suicide notes(1), the reasons given in the note for the suicide act were classified. The question may be raised whether credence can be given to the reason in the note since the individual may have been too disturbed to give a clear account of the circumstances leading to the suicide act. The purpose of this study, therefore, is to estimate whether the note accurately reflects the situation, by comparing the reason given in the note with that elicited from relatives, friends, or family doctors in the official investigation of the suicide by the Office of the Medical Examiner.[4] Although the degree of objectivity may vary among these informants, presumably they represent more reliable sources of information than the suicide himself.

In the study referred to above, notes were left by 165 suicides. Thirty percent of the notes gave no clue as to the reason for the suicide; in 32% of the cases the record showed no informant. The number of cases in which reasons for the suicide were available from both note and informant was 63. In 39 cases there was one informant; in 24 cases there were 2. The total number of informants was 87: 15 (mostly physicians) unrelated to the suicide, 53 close relatives (spouse, children, parents, sibling), 18 more distant relatives (nephew, niece, in-laws), and 1 with relationship not stated.

PROCEDURE AND RESULTS

Each of the 3 authors independently listed the reasons given in the 63 notes. Using the paired-agreement method, the amount of agreement among any 2 raters varied from 84% to 91%, the amount of partial agreement from 3% to 8%, and the amount of disagreement from 3% to 6%. To compare the reason given in the note with that given by the informant, it was necessary to have agreement on all notes. Therefore, the raters jointly reviewed any notes on which there was not full agreement. The reason agreed upon was then compared with that given by the informant, as stated in the records of the Office of the Medical Examiner.

In general, the reasons given by informants were more specific than those found in the notes. This is understandable since reasons given by informants are responses to direct questions by official investigators regarding circumstances leading to the suicide. The writing of the note, on the other hand, is an unstructured situation; no request was made of the individual to produce a note and there were no norms to guide him about its content. Some individuals gave detailed information, readily understandable by anyone reading the note, about their life situation and the factors that played a part in their decision to kill themselves. More often the information was somewhat obscure, but presumably clear to the individual to whom the note was addressed. For example, a note from a wife to her husband might not and would not need to contain a detailed account of the precipitating circumstances already known to him. In such cases, the note might just refer to the fact that she could not go on living under these conditions.

The comparisons of reason in the note and that given by the informant were classified into 3 categories: agreement, compatibility, and disagreement. The second category was necessary because some cases could not be classified in terms of agreement or disagreement, perhaps owing to the difference in the structuring of the situation for note-writer and informant. Examples of each of the 3 categories are shown below. For each category, the complete note is reproduced with the names omitted. The reason in the note as agreed

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upon by the 3 raters and that given by the informant are also shown.

1. Agreement

I am losing the sight of both my eyes. Please take good care of the dogs. I have a heavy overcoat and trousers being cleaned at F—— on Ogontz, above 67th.

Reason agreed upon by raters–Losing sight in both eyes.

Reason given by informant–Brother: Eyes bothering him and was in poor health.

2. Compatibility

Dear D—— and D—— :

I didn't know how you felt. I am sorry for anything I caused. I have always enjoyed card games with you and D——, when you and he liked to play. But I guess I expected to much. I have always been afraid to be alone in the Eve's, that's my only fear, loneliness. Each day I looked forward to you and D—— coming home in the Eve. I am sorry believe me This way will be better for you & D——. I don't want to stand in your way.

The house will be yours and D——'s equally to live in as long as you 2 wish I'm no good by myself & don't want to stop you and D—— from having fun. So this is the only thing I can think of.

I never realized before how much I was in the way. God bless and keep you both safe. All my love.

Mother

Reason agreed upon by raters–Desire not to stand in children's way, loneliness.

Reason given by informant–Children : Despondent since the death of her husband 2 years ago, and was under doctor's care.

3. Disagreement

My soninlaw is the cause of this.

T—— ——

Reason agreed upon by raters–Apparent difficulty with son-in-law.

Reason given by informant–Daughter : Was depressed since death of wife a year ago.

Table 1 presents data on the amount of agreement, compatibility, and disagreement between note and informant. Since there were 24 cases with 2 informants, 2 comparisons are shown : one utilizing the first informant mentioned in the case record, the other utilizing the second. In each comparison, the 39 cases with just one informant were included.

Considerable agreement between the note and informant is evident : in each comparison there is 75% agreement and, in addition, 18% compatibility. Only 7% of the cases show disagreement.

It may be noted that the comparison between informants 1 and 2 (24 cases), also presented in Table 1, shows somewhat less agreement than between note and informant, but the difference is not statistically significant. The compatibility between reasons given by 2 informants may be due to variation in familiarity with the precipitating circumstances rather than to the difference in the structuring of the situation.

Discussion and Conclusion

The data indicate clearly that credence can be given to the reason found in the suicide note. This conclusion is warranted

TABLE 1

Comparison Between Note and Informant and Between 2 Informants

Category	*Note and Informant*		*Informants 1 & 2*
	*Comparison 1**	*Comparison 2**	
	n=63	*n*=63	*n*=24
	%	%	%
Agreement	75	75	63
Compatibility	18	18	21
Disagreement	7	7	16

* Comparison 1 utilized the first informant in the 24 cases with 2 informants, and comparison 2 utilized the second informant in these cases.

because in over 90% of the cases there was agreement or compatibility between the reason in the note and that obtained from the informant. In addition, the amount of agreement between note and informant is as high as that found between 2 informants. The agreement between note and informant cannot be attributed to the possibility that the informant had access to the note and merely repeated the reason in it. Assuming easiest access to the notes for close relatives, less for more distant relatives, and least for individuals unrelated to the suicide, comparison of note and informant showed no difference among the 3 groups in the amount of agreement, compatibility or disagreement. Moreover, it is unreasonable to expect that the informant, presumed to be a normal individual, would give the same reason as that found in the note unless he thought it to be correct. Even if the informant did subscribe to the distorted perceptions of an individual who killed himself, the high amount of agreement between note and informant suggests that as much confidence can be placed in the note as in the informant.

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CLINICAL NOTES

A CLINICAL NOTE ON ISOCARBOXAZID

VINCENT T. LATHBURY, M.D.[1]

Careful detailed observations were made on 14 private patients unresponsive to psychotherapy alone. Because of ideal conditions, frequent visits, and duration of continous treatment, it is felt the results have a definite clinical value.

It was concluded that when administered to patients in certain categories, isocarboxazid[2] is an extremely safe and effective antidepressant. Because it is effective, however, there may be a tendency to use it in less typical cases with uniformly disappointing results.

The usual starting dose was 30 mg. daily, with later reduction to 20 or even 10 mg. daily.

Uniformly excellent results were observed in depressed but non-psychotic patients. The typical complaint was a feeling of depression accompanied by difficulty in making decisions and a sense of inferiority, inadequacy and inability to perform, and occasional ideas of suicide. Here isocarboxazid was very effective in relieving depression, but as one would expect, the underlying neurotic process remained unaltered.

One 56-year old, married, childless female had suffered for 3 years from an incapacitating depression that occurred every other day with such regularity that following a major operation it remitted for about 3 weeks and then resumed its course in exactly the same cycle.

Two courses of ineffective electroshock therapy were followed by two years of psychotherapy with some increased insight but no genuine clinical improvement. Given iproniazid, she experienced improvement for the first time. After 3 months she was switched to isocarboxazid, principally to lessen the possible risk of liver damage because of previous infectious hepatitis. Her improvement was so marked that she felt her old self again. Almost a year later this improvement still obtains.

The comparative safety of isocarboxazid is illustrated by the case of a middle-aged man with a chronic compulsive neurosis. During analysis he developed a very severe depression which was unresponsive to analysis or psychotherapy.

The effects of iproniazid were so dramatic that he was soon able to return to his position as a business executive. After about 6 weeks, however, he developed hepatic complications, the drug was discontinued, and within 2 weeks a complete relapse had taken place. Isocarboxazid was instituted and in 2 weeks there was a complete remission. Four months later the patient is still taking the drug with no clinical or laboratory signs of any hepatic toxicity. Although this is only one case, it would seem to indicate that the drug must be extremely well tolerated by the liver.

Two cases of reactive depression were treated. One man had an alcoholic wife and the other's wife was chronically nagging, quarrelling and perpetually dissatisfied. Both patients were restored to normal mood and function within 10 days on a dosage of 10 mg. isocarboxazid t.i.d.

Two elderly females with addiction problems (tranquilizers and chloral hydrate) became depressed after withdrawal of medication and a week of withdrawal symptoms. Isocarboxazid was administered in the usual daily dosage. About a week later the depression was relieved and both were able to resume their former interests and activities.

Isocarboxazid seems to be most useful in patients who exhibit both motor and psychic retardation, but it is often effective in those who also have considerable tension and agitation.

The only consistently observed side effects were increased muscle tension, insomnia and transient attacks of dizziness, presumably due to vascular hypotension. The former were in most cases not severe

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enough to warrant therapy, but if necessary they were readily controlled with moderate doses of sedatives or tranquilizers. Dizziness was easily managed if patients were cautioned against sudden changes of position, and advised to sit down for a moment until the attack passed.

SUMMARY

Careful detailed observations were made on 14 private patients unresponsive to psychotherapy alone. It was concluded that when administered to patients in certain categories, isocarboxazid is an extremely safe and effective antidepressant.

THE ESTIMATION OF PHENOTHIAZINES USING CHEMICALLY IMPREGNATED PAPER STRIPS

JACK J. HEYMAN, M.S., BARBARA BAYNE, B.S., AND SIDNEY MERLIS, M.D.[1]

Methods for the rapid estimation of phenothiazines have been developed by Forrest(1, 2, 3, 4, 5). In our laboratory we have investigated the use of treated paper strips for these determinations. In a previous publication(6) we described a $FeCl_3$-impregnated sulfonic acid resin-impregnated test strip. Lin(7) modified this to a $FeCl_3$-impregnated Whatman 3MM paper. However, the yellow background of these papers does not make slightly positive results readily ascertainable. We have therefore developed other strips to remedy this difficulty.

MATERIALS

From rolls of Whatman 3MM, ½" to 1" wide, 7" strips were cut. The paper was treated with either .001M mecuric nitrate (343 mg. $Hg(NO_3)_2.H_2O$/1 H_2O) or .001M Uranyl nitrate (502 mg. $UO_2(NO_3)_2.6H_2O$/1 H_2O) by immersion in the solution. The paper was placed on clean paper toweling and air-dried.

The ammonium persulfate treated strips were made by the addition of 90 g. of ammonium persulfate to one liter of .001M mecuric nitrate or one liter of the uranyl nitrate solution. The paper was then treated with these solutions. The strips may be used without drying. They are not stable for more than a few days when dry and should be discarded when they become discolored.

METHODS

Urine samples were tested by placing a drop of urine on the paper. A drop of 3% H_2O_2 was applied to the same spot. After a few moments a drop of concentrated HCl was applied to the spot. When the ammonium persulfate paper was used the H_2O_2 was omitted. A negative result gave no color. A positive result gave a violet color. The colors were graded against the color chart present in Figure 1.

To test urine in suspected cases of overdose the procedure is changed slightly. The acid is placed adjacent to the sample spot after the peroxide addition. With a negative sample the paper remains white. A positive result with unmetabolized phenothiazines is red to red-brown. The amount of drug present was estimated by comparison to a color chart. The chart presented in Figure 1 was developed for the ammonium persulfate treated strips. When the H_2O_2 is used, slightly lighter color develops because of the spreading of the sample. The estimate was also carried out by running serial 1:1 dilutions with distilled H_2O until a negative result appeared. The "approximate minimum detectable quantities (AMD)" of several phenothiazines are presented in Table 1. The AMD multiplied by the dilution factor gives the estimated drug concentration in milligrams/milliliter:

2^n x AMD=concentration, where n=# of the tube in 1:1 dilution series.

The usefulness of the strips in the testing of urine samples was examined. To check our results, the Forrest Universal(5) test and the Forrest test for piperazine-linked phenothiazines(4) were run on the same samples.

[1] From the clinical facilities and research laboratory of the Research Division, Central Islip State Hospital, Central Islip, N. Y.

PHENOTHIAZINE TEST

SAMPLE			
URINE grade	2	3	4
SPARINE mg./ml.	1.56	.78	.19
TRILAFON mg./ml.	.31	.15	.08
COMPAZINE mg./ml.	.62	.31	.15
THORAZINE mg./ml.	.39	.098	.049
VESPRIN mg./ml.	2.5	1.25	.62
STELAZINE mg./ml.	1.0	.5	.25

TABLE 1

THE AMD'S OF VARIOUS PHENOTHIAZINES IN MG./ML.

Test	Hg, H_2O_2	Hg, $NH_4S_2O_8$	U, H_2O_2	U, $NH_4S_2O_8$	Fe, SA-1
Thorazine	.024	.024	.024	.048	.031
Stelazine	.031	.062	.062	.062	.250
Vesprin	.019	.019	.019	.019	.031
Sparine	.048	.048	.048	.048	.015
Trilafon	.039	.019	.039	.039	.009
Compazine	.038	.038	.038	.038	.031

RESULTS

In a series of 400 urine samples from geriatric patients, approximately 15% false positives with a graded color of one or more were observed with the strips and both Forrest tests. The high rate of false positives is, in part, related to elevated levels of bile pigments as pointed out by Forrest. The false positives could not be correlated with the intake of other drugs.

The overall agreement of the Forrest tests with the strips was of the order of 86% agreement.

In a series of 19 chronic schizophrenic patients, who had not been on any phenothiazines in several months, the same series of tests were run. These results (Table 2)

TABLE 2

COMPARISON OF THE RESULTS WITH TEST STRIPS AND FORREST TESTS

Test	*Negative*	*Grade +1*	*Grade +2*
U/H_2O_2	11	7	1
Hg/H_2O_2	11	7	1
$U/NH_4S_2O_8$	11	7	1
$Hg/NH_4S_2O_8$	11	7	1
Forrest Universal	6	11	2
Forrest piperazine-linked	7	10	2

were used as control values. This group of patients was retested after phenothiazine therapy was instituted. Five patients failed to show an increased color development with the strip tests and the Forrest tests during the first week of treatment. Three of them received 15 mg./day of Stelazine. The other two received 50 mg. of an experimental phenothiazine, WY-2445.[2]

[2] This compound was supplied through the courtesy of Wyeth Laboratories, Philadelphia, Pa.

An additional 31 chronic schizophrenics were tested after drug therapy was started. Of this group, 9 failed to show positive results with the Forrest tests or the strips in their morning specimens. Specimens were taken 3 hours after the A.M. drug administration. The amount of Stelazine given was 5 mg. and the amount of WY-2445 was 50 mg. When 3-hour urine samples were tested, positive results were consistently observed.

CONCLUSION

The Forrest tests and the test strips give essentially the same results. These methods for testing for phenothiazines excreted in the urine require some caution on the part of the observer. A positive test may be indicative of phenothiazine intake, but also may be due to bile pigments or other excretion products. A negative test may be indicative of no drug intake or low intake. When interpreted with caution, these tests remain valid for estimating phenothiazine intake.

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IMIPRAMINE THERAPY OF DEPRESSIVE SYNDROMES

LEON REZNIKOFF, M.D.[1]

In recent years numerous reports appeared in psychiatric literature abroad and in this country dealing with the use of new pharmacological agents in the treatment of depressive states; several monoamine oxidase (MAO) inhibitors have been found helpful in alleviating depressions.

However, these investigators in discussing MAO inhibitors emphasize the need for frequent and periodic laboratory studies of liver function; these tests while very useful in determining the slightest damage to the liver, frequently alarm the already apprehensive and hypochondriacal patients to such degree that the effectiveness of the drug may be nullified by emphasizing the possibility of body damage.

The therapeutic usefulness of the MAO inhibitor drugs in depressions had been established beyond any doubt but the drawback of possible liver damage makes it difficult for the clinician to prescribe these drugs for some ambulatory patients.

Therefore, a drug which is not a MAO inhibitor, and is not likely to produce serious side effects should be most useful in the treatment of ambulatory depressive patients.

During the past year I have been using imipramine hydrochloride (Tofrānil)[2] in the treatment of depressions. It is not a MAO inhibitor and according to our present knowledge is not apt to produce any liver damage. This brief report deals with the treatment of 40 depressed patients divided in two groups: the first group consisted of 25 ambulatory patients treated in a private office; the second consisted of 15 patients committed to a public mental hospital. They belonged to the following diagnostic classifications:

Ambulatory group: endogenous depression, 13; reactive depression, 6; agitated depression, 6.

Hospitalized group: manic-depressive reaction, depressed, 8; involutional psychotic reaction, 2; schizo-affective reaction, 5.

The youngest patient was 24 years old, the oldest 75. There were 15 male, and 25 female patients.

Extensive laboratory studies have been carried out for the hospitalized group; transaminase tests for liver function and blood counts were performed at weekly intervals; patients have been closely observed for any side effects. Blood pressure and weight recorded weekly. The most frequent complaints consisted of, in the following order of frequency: dryness in the mouth, profuse perspiration, constipation, dizziness, blurred vision and hot flushes. One patient during the course of treatment with imipramine developed a mild hypomanic state, which subsided in about 2 weeks. Since imipramine produces atropin like effects it is contra-indicated in glaucoma. When there was gain in weight it was attributed to the alleviation of depression and return of appetite, rather than any special side effect of imipramine, since patients who failed to improve clinically, also failed to gain weight although they had been on imipramine for over 3 months.

The treatment was started with 25 mg. 3 or 4 times a day and increased to 150 mg. per day; only a few patients required larger doses; maximum dose used in 2 cases amounted to 225 mg. per day in divided doses.

Most of the ambulatory patients had anxiety elements in their clinical picture of depression; addition of a tranquilizing drug to the imipramine regime during the day, and barbiturate at night to facilitate sleep had been found most effective.

All patients were interviewed at least twice a week during the first 2 or 3 weeks on therapy; after that at weekly intervals; psychotherapy of supportive type was employed with all hospitalized patients; the ambulatory patients received directive psychotherapy. Thirty-two (80%) patients in both groups either achieved complete remission, or improved to such extent that

[1] Clinical Director, Hudson County Hospital for Mental Diseases, Secaucus, N. J.

[2] Imipramine hydrochloride generously supplied for this study by Geigy Pharmaceuticals under name of Tofrānil.

they had been able to return to their former occupation. Eight patients (20%) either showed slight improvement or transient changes, and therefore were classified as unimproved. Because of tendency of depressive patients to relapse, treatment with imipramine was carried on for at least 3 months, although the dose was reduced 2 weeks after patients achieved what seemed to be maximum improvement; however with some patients improvement was so marked, that imipramine could be discontinued after 2 months.

Summary and Conclusions

Forty patients suffering from various depressive syndromes had been treated with imipramine hydrochloride for a period of 3 to 12 months.

Remissions and marked improvement had been obtained in 80% of cases.

The effect of the drug is apparent in 2 to 4 weeks after beginning of therapy, and in some cases it is noticeable even after a few days.

Imipramine is effective regardless of long duration of the depression. The relief of depressive feelings is not dramatic and sudden, as with ECT, but rather gradual.

In refractory patients with a tendency to relapse, the drug had been administered in reduced dosage for 12 months, and apparently can be continued indefinitely; this is a distinct advantage over ECT, since ambulatory patients inevitably after a few courses of ECT resist further attempts at maintenance, or preventive ECT.

In none of the 40 patients did imipramine have to be discontinued because of side effects, although several patients complained of dryness of the mouth, profuse perspiration, constipation, dizziness, blurred vision and hot flushes.

TREATMENT OF CHRONIC SCHIZOPHRENICS WITH LIOTHYRONINE (L-TRIIODOTHYRONINE)[1]

E. J. TOLAN, M.D., B. KOVITZ, M.D., and LOWELL DILLON, M.D.[2]

Liothyronine is a potent hormone with qualitative metabolic and physiological effects of desiccated thyroid and L-Thyroxin, differing only in its chemical structure, previously used in the treatment of low basal metabolism without myxedema, in obesity, cretinism, sterility, alcoholism, and mental disorders.

Some authors have reported relief of mild to moderate ambulatory depressive states but could not explain why some patients responded while others did not.

Twenty-four hospitalized chronic schizophrenics were selected for treatment. Ages varied between 24 and 48 years (average 36.7 years). Duration of illness 1 to 31 years (average 8.5 years).

Despite different admission diagnoses, symptoms of: withdrawal, unsociability, seclusiveness, inactivity, depression, and uncooperativeness, varied. Some were unkempt, negativistic, taciturn, at times mute, with minimal signs of overt anxiety. All had previously received tranquilizing medications without major or lasting improvement; 21 patients had previously received electro-convulsive therapy and 9 insulin therapy, with only temporary benefit.

C.B.C. and urinalysis were performed on all cases, cardio-vascular disease was ruled out. P.B.I., B.M.R., and serum cholesterol were not determined; our chief interest was the mental status of the patients. Vital signs and weights were checked at regular intervals. All were free of physical disease except one male and one female patient who had shown thyroid insufficiency on previous examination. One had been treated with thyroid, the other received thyroid 1 gr. daily concurrently with the liothyronine.

Each patient served as his own control, receiving divided daily oral dosage, over a 13-week period, the smallest dose 10 mcg.,

[1] "Cytomel" trademark for liothyronine (triiodothyronine), furnished by the courtesy of S.K.F. Lab.

[2] Respectively: Resident Psychiatrist, Clinical Director, and Superintendent, Columbus State Hospital, Columbus, Ohio.

the largest 100 mcg., with a total of 620 mcg.

Physical Effects: Vital signs stayed within normal limits, no signs of cardiac failure were noted. There were no reports of dizziness, headaches, excessive sweating, pruritus, menstrual disturbance, or excessive urination. Some patients showed an increased appetite; 8 gained weight, 12 lost and 4 stayed the same.

Six showed minimal side effects as: restlessness, tension, anxiety, fear, and sleeplessness. Five females showed extrapyramidal syndrome signs, with rigidity, shaking of extremities, staring looks, mask-like facies and dragging of feet. They also showed marked side effects and mental confusion and medication was discontinued. Thirteen patients were free of any side effects.

Psychological Results: Of the 24 patients; 8 showed improvement (cyclic), 12 no change, and 4 were worse. This cyclic improvement had never been previously observed. The degree of improvement can only be described as minimal. During the periods of clinical improvement, the patients were more alert, active, social, and talkative. They paid more attention to themselves and their surroundings. Some became more industrious. Depression and withdrawal were less noticeable. In general, the improvement was not only periodic but was noted chiefly during the peak dosage, and diminished or disappeared as the medication was reduced.

In the 4 patients who appeared worse, restlessness, sleeplessness, tension, lack of interest in themselves or their surroundings, and depression were noted. Delusions and hallucinations appeared intensified and were more readily expressed. Two patients were also hostile, irritable, fearful, and uncooperative; 4 patients, including 2 of the improved group displayed overt sexual interest toward other patients, nursing staff, physicians, and relatives.

Changes for the worse occurred at any time after the first two weeks of treatment.

Summary and Conclusion

Half of the group of 24 showed temporary changes. These changes were maximal at the peak doses, either in the direction of decreased depression and withdrawal (8 cases) or increased restlessness and tension (4 cases). These changes were not lasting, and as the medication was decreased, the group as a whole returned to its original level. The temporary periods of improvement occurred in a peculiar cyclic pattern, lasting from a few days to a few weeks at a time.

In higher doses the drug was accompanied by changes in the mood and activity of chronic schizophrenic patients, but did not bring about a lasting or dependable improvement in psychiatric status or hospital adjustment. The use of liothyronine in psychiatric disorders must still be considered in experimental stage.

DR. UGO CERLETTI

HISTORICAL NOTES

THE STORY OF THE FIRST ELECTROSHOCK TREATMENT

DAVID J. IMPASTATO, M.D.[1]

I recently asked Professor Ugo Cerletti of Rome, Italy, to tell me the story of the first electroshock treatment.

Prior to assuming the professorship in psychiatry in Rome in 1935, Cerletti had for a number of years been investigating histopathologic cerebral changes consequent to convulsions in animals. To avoid artifacts, from toxic substances or from the passage of electricity through the brain, he did not use drugs to produce the convulsions and placed the electrodes one in the rectum and the other in the mouth (Viale method). This method did not entirely prevent electricity reaching the brain as was later shown by Bini. With the Viale method, not a few of the dogs died from cardiac arrest as the current traversed the heart. To avoid this complication, convulsions were produced with the least possible quantity of electricity given for a very short time (60-70 volts for 0.1 second).

Soon after Meduna published his experiences with Cardiazol Convulsive Therapy in Psychiatric Conditions, Cerletti introduced this therapy in Rome. It then occurred to almost all those in his group who were daily inducing electric convulsions in dogs, to apply this method therapeutically to man. Most of the researchers, however, were timid and feared causing death, irreversible brain changes and epileptic states. Cerletti was the least fearful, but as yet he did not dare to initiate the procedure. Later seeing a parallel between the cardiazol convulsion and the convulsions caused by transcranial application of electricity; using a bi-temporal application of the electrodes, he experimented on many pigs which were placed at his disposal at the slaughter house in Rome. With these animals he changed the scope of his experiment and instead of using the least amount of current to produce the convulsion, he set out to find the quantity of current needed and for how long a period of time it should be applied to kill an animal. After noticing that in order to do this a tremendous amount of current had to be used for a prolonged time, and that there was a vast difference between a convulsant and a killing dose of electricity, he became certain that the method would be safe in man and decided to go on with it. This was his decision and no one else had anything to do with this aspect of the procedure. Cerletti asserts that EST was not an invention but it was merely an audacious act. He gives to Meduna the honor of having invented the convulsive therapies.

Bini together with the electrical engineer of the clinic constructed the machine which had two circuits:

A direct circuit for the measurements of the resistance of the patient's head, measured in ohms. The other, an alternating current to elicit the convulsion. This circuit included a timer which measured time in 1/10 of a second up to a minute; a potentiometer which allowed the voltage to vary from 50 to 150; and an ammeter to indicate the milliamperage which flowed between the electrodes. The circuits were contained in a metallic case which made the apparatus quite heavy. Dr. Renato Almansi who worked with Dr. Cerletti, brought one of these machines to America in 1939 which he and I used in our experiments on dogs, and in our first patient.

Now came the search for Rome's first patient. For obvious reasons this was not a simple matter. Then, luckily, a patient from North Italy was admitted to the clinic who was a catatonic schizophrenic and who spoke an incomprehensible gibberish. He was unable to give his name or to state anything about himself. No one could identify him. Dr. Cerletti decided he should be the historic patient. Following

[1] 40 Fifth Ave., New York 11, N. Y.

adequate preparations the first treatment was given in 1938. Present were Cerletti, Bini, Longhi, Accornero, Kalinowsky and Fleischer. The patient was brought in, the machine was set at 1/10 of a second and 70 volts and the shock given. Naturally, the low dosage resulted in a petit mal reaction. After the electric spasm, which lasted a fraction of a second, the patient burst out into song. The Professor suggested that another treatment with a higher voltage be given. The staff objected. They stated that if another treatment were given the patient would probably die and wanted further treatment postponed until the morrow. The Professor knew what that meant. He decided to go ahead right then and there, but before he could say so the patient suddenly sat up and pontifically proclaimed, no longer in a jargon, but in clear Italian : "Non una seconda ! Mortifera !" (Not again, it will kill me). This made the Professor think and swallow, but his courage was not lost. He gave the order to proceed at a higher voltage and a longer time : and the first electroconvulsion in man ensued. Thus was born EST out of one man and over the objection of his assistants.

COMMENTS

PROFESSIONAL ETHICS FOR THE PSYCHIATRIST IN THE PRESENT DAY [1]

Advances in science and methodology through the centuries have radically and continuously changed the practice of medicine. The fundamental philosophy of medical practice has not changed to any significant extent, however. The Code of Medical Ethics of the American Medical Association, first published in 1848 and revised six times subsequently, including the most recent revision in 1955, still embodies in principle the standards of ethical conduct for physicians which Hippocrates included in his renowned oath. Now as in the days of Hippocrates and his followers, the chief concern of the physician in the practice of his profession is the benefit of his patients. Certainly he is expected to "abstain from every voluntary act of mischief and corruption; and further, from the seduction of females or males, . . ."[2] To quote from the Hippocratic Oath, the confidential relationship between the physician and his patients remains a sacred trust, and the right of privileged communication has been upheld by law.

Psychiatry is a special branch of medicine. Physicians for many years have assumed direct responsibility for the care and treatment of the mentally ill. The first national medical association in North America, was organized by thirteen physicians who were in charge of institutions providing residential care for mentally ill patients. This group of physicians, in their meeting in Philadelphia, Pennsylvania, on October 16, 1844, established the Association of Medical Superintendents of American Institutions for the Insane. In 1892 the name of this organization was changed to the American Medico-Psychological Association. In 1921 the Association was renamed The American Psychiatric Association, and in 1927 it was incorporated under the laws of the District of Columbia.[3]

On May 5, 1951, the Council of the American Psychiatric Association approved *A Manual of Organization and Policy* (Presenting Our Purposes and How We Work Toward Them). This manual was approved by the membership of the Association on May 8, 1951, at the annual meeting in Cincinnati, Ohio. Section VI of the *Manual*, which concerns professional ethics, notes that "The APA recognizes and adopts the Code of Ethics"[4] of The Canadian and The American Medical Associations.

A code of ethics for psychiatrists was drafted in 1953 by members of the American Psychiatric Association after several years of study had been given the matter. This proposed code presented certain special problems of concern to psychiatrists which had not been included in the American Medical Association Principles of Medical Ethics. This proposed code was considered by several district psychiatric societies and adopted by one. The Council of APA received the proposed code of ethics in 1955 but decided to study the matter further inasmuch as there was no consensus among the membership that psychiatrists needed a code of ethics separate from that to which other physicians subscribe. There has been no further action to date regarding a special code of ethics for psychiatrists.

Some years ago the American Psychiatric Association assigned to a standing committee, the Committee on Ethics, the responsibility of investigating complaints and accusations presented against psychiatrists who are members of the Association. The Council of APA in 1955 adopted a Code of Procedure to follow in regard to matters of ethics recommended by the Committee

[1] At the request of the Journal, Dr. Tarumianz, who is chairman of the Committee on Ethics, kindly prepared this statement which we are happy to print as a guest editorial comment. Ed.

[2] Oath of Hippocrates, quoted from M. A. Tarumianz, "History of Medical Ethics, *Delaware State Medical Journal*, Vol. 21, No. 10, October 1949, p. 225.

[3] A Manual of Organization and Policy, *American Psychiatric Association*, p. 1.

[4] Ibid., p. 5.

on Ethics. According to the procedure adopted, the Committee on Ethics makes the investigation of charges. A hearing on hearings may be arranged at which the accused psychiatrist and/or his counsel may appear; after receiving the report of the findings and recommendations to the Committee, the Council of the APA takes final action.

A procedure for disciplinary action against members of the Association who are proven guilty of violations of medical ethics was adopted by the Council in 1957. In 1958 the membership voted amendments to the Constitution and By-Laws of the APA, giving to the Council authority by which "a member may be admonished, reprimanded, expelled or suspended from the privileges of membership if such action is determined and voted by two-thirds of the Council; provided Council, by a two-thirds vote, shall determine that such a member has been engaged in unethical or unprofessional conduct, or has wilfully refused to comply with resolutions or requests of the Council, or brings discredit or dishonor on the Association or on the practice of psychiatry, or if he has been convicted of a crime involving moral turpitude."[5]

In the discussion which preceded the adoption of procedure for disciplinary action against members, there was some question as to the necessity for such provisions. Although psychiatrists have not lacked definitely stated principles of professional ethics to guide them, it appears that the conduct of some members of the profession has led to questions and accusations suggesting possible violation of ethics. During each year in which the writer has served with the Committee on Ethics, both as a member and more recently as chairman, complaints and accusations have been presented against members of the Association. Some of the complainants have been former patients of the accused physicians, but some have been other physicians.

In several instances physicians have been involved in publicity suggestive of self-advertising. To promote the sale of a physician's book, publishing companies, sometimes acting without the author's approval of knowledge, may circulate brochures which make unwarranted claims or are written in a style not in keeping with the dignity of the physician-author. An author can not abrogate all responsibility for the type of publicity his production receives. He must reserve the right to approve the publicity material to be used in presenting his writings to the public.

At times physicians have been quoted in advertisements of pharmaceutical products. A physician engaged in experimental study of various medications would be expected to report in scientific journals or before professional societies the results of his research. He would not endorse particular pharmaceutical products.

The preparation of a psychiatrist is costly in both time and money. Higher fees may be justifiable for the services of this type of specialist, but unreasonably large fees or unwarranted claims for cures can not be supported. Several complaints of malpractice have been argued in courts of law in various states, and the Committee on Ethics has received accusations against some physicians who, in return for exorbitant fees, guaranteed to cure a patient of a mental condition for which there is as yet no known cure. Charges have been brought for the alleged promise of a physician to treat one mentally ill patient exclusively and receive a large fee on a regular basis. Professional ethics demands of the physician a realistic and *humble* appraisal of his abilities and limitations in trying to meet the needs of other individuals. Also, the dedication of the physician must be to render service to ill people rather than to make money.

The apparent impetus to practice psychiatry for monetary gain *per se* has led to accusations against physicians of adopting practices suggestive of defrauding rather than serving the public. Care should be exercised in the involvement of non-medical personnel in the treatment process. The position of the American Psychiatric Association is quite clear that "(1)'Psychotherapy is a form of medical treatment and does not form the basis of a separate profession . . .' (2) It is imperative that all psychologists and other non-medical personnel dealing with persons suffering from mental and nervous disease and disorder

[5] APA By-Laws, Art. III, Sect. 3.

should do so only under supervision by psychiatrists and in a medical setting offering adequate safeguards to the patients."[6]

The charge of the violation of privileged communication has been made against some psychiatrists. Even in situations in which the release of confidential information is to advance the treatment of a patient, as in the case of requests for past histories or diagnostic summaries to be sent to hospitals or other physicians, permission should be obtained from the patients, if they are competent, or from responsible relatives or guardians before such information is released. As the *Code of Ethics* of the American Medical Association indicates, "Sometimes, however, a physician must determine whether his duty to society requires him to employ knowledge, obtained through confidences entrusted to him as a physician . . . Before he determines his course, the physician should know the civil law of his commonwealth concerning priviledged communication."[7]

The most serious accusation, and the one presented most frequently by patients or their relatives, is that of alleged intimacies between physicians and patients. Physicians, particularly psychiatrists, are in a vulnerable position. Much of their practice involves the treatment of emotionally disturbed or mentally ill persons who may misunderstand or misinterpret the physician's relationship. The physician must be constantly on guard against any emotional involvement of a patient with him. He should exercise care concerning the circumstances under which he treats a patient. His treatment methods should be those approved by the medical profession and especially those which would be accepted by society in general.

Various theories of treatment may be suggested in good faith, but the psychiatrist should be wary of any which would be questioned by his professional colleagues especially.

Theories and practices in medicine, and in that phase of medical specialty called psychiatry, will continue to change, no doubt. The high principles of medical ethics will remain constant. The physician's chief reason for the acquisition of training and skill as well as for the practice of his profession is to do all in his power to relieve the ills of humanity. The dedication of physicians to high ethical standards of medical practice is essential if the profession is to advance.

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[6] Principles of Medical Ethics of the American Medical Association, Chapter II, Sec. 1, Chicago, Illinois, p. 14.

[7] APA Manual, *op. cit.*, pp. 5f.

RULES

The young man knows the rules, but the old man knows the exceptions.

—Oliver Wendell Holmes

CORRESPONDENCE

PSYCHIC DETERMINISM: AN OUTMODED CONCEPT?

Editor, THE AMERICAN JOURNAL OF PSYCHIATRY:

SIR: In the November issue of the *Journal,* Ronald W. Angel criticizes the notion of psychic determinism on the basis of recent developments in physics. He notes that psychic determinism has often been considered fundamental to psychoanalysis, that Freud frequently treated it as an axiom on which investigation was based, and that some contemporary analysts, even after losing faith in it as an axiom, are reluctant to abandon it as a healthy working hypothesis.

On the other hand, Angel calls attention to the discovery by physicist Werner Heisenberg that the smallest particles behave in a random fashion. The behavior of these particles can be predicted only when they are considered in the aggregate, according to statistical calculations of probability, the prediction taking the form of an anticipation with a high probability rather than an assurance.

In attempting to understand the hold which the notion of psychic determinism has on certain thinkers, Angel cites with approval Bertrand Russell's contention that determinism has become ingrained over the centuries, while the newer physics is difficult to understand. Still following Russell, he translates the concept of determinism from a statement about causality into a statement about a functional relationship which says, in effect, that the state of a system at a given time is a function of its states at previous times. The determinists, according to Angel, believe that such a function is "limited in complexity; capable of being apprehended and written down." Angel believes that with this qualification Russell's formula expresses the determinism position, and manages to do so without any reference to some eternal force of causality. Instead of an axiom of investigation, it is merely a hoped-for regularity which may or may not be found as science learns more about the world.

Angel then suggests that in fact we now have some reason to think that the hope is unfounded; the regularity is not there; determinism does not hold; there is randomness in the workings of the mind.

He notes that even Freud entertained the idea that an organic rather than a psychic state may account for a psychic effect, and he refers to a recent suggestion that at a decisive point in neuronal pathways, psychological results may be determined by structures small enough to "come under" the Heisenberg Principle, in other words, psychological states may represent random neurological activity. He suggests that eventually a "statistical point of view" may replace determinism in the realm of psychology.

Angel has done a service in confronting psychology with the new physics; habitual modes of reasoning should be disturbed and challenged by the recent developments in the most advanced science. The purpose of this paper, however, is to underline the tentativeness with which Angel's conclusions are presented, and to support the thesis that the lesson for psychology to learn from modern physics is not yet available. Angel, after all, is dealing with several of the most intractable headaches of philosophy, and one cannot be too cautious in evaluating the claims of a new remedy for a malady of hundreds of years duration. (Among the symptoms are problems of causality and chance, the mind-body problem, the problem of substance or objects, and the problem of the *a priori.*)

The main points of Angel's presentation, as summarized by him, are: 1. That physics no longer accepts causality as a basic postulate; 2. That psychoanalysts have been reluctant to abandon it because the idea has become habitual to them, while the alternative is difficult to understand; 3. That determinism can be expressed as a functional relationship, and 4. That this functional relation is open to question, since it cannot be established *a priori.* These points will now be briefly debated.

HAS PHYSICS DISPROVEN CAUSALITY?

Physics *per se* makes no statements about causality; modern philosophy of science makes many. Just as there are varying schools of psychology, so there are varying schools of philosophy. One must be careful to guard against assuming that recent theories are necessarily more adequate than older ones, just as one would hesitate to pronounce existentialism an advance beyond psychoanalysis, or neo-Zen an improvement on existentialism on chronological grounds alone. More important still, history shows that philosophy of science, far from directing scientific thought, is a follower and explainer, and tries to make general sense out of specific scientific advances. It is never as up-to-date as the contemporary science it studies, so that even the legitimate enthusiasm for the latest theory should be tempered by the fact that it is young and immature.

Statements in modern physics tend to take the form of symbolic relations between symbolic notations. It has been the difficult task of the philosophy of science to elaborate the significance of these symbols. No one will say that this task has been completed. Bertrand Russell, in fact, is among those who have been actively wrestling with the translation of scientific statements into the terms which we ordinarily use to describe our world. Among philosophers, even among those of kindred outlook, there is probably more disagreement than agreement on this subject. We must note, then, that the Heisenberg Indeterminacy Principle applies to "particles" the nature of which cannot as yet be confidently stated apart from the symbolic notation in which the principle is formulated; we have not even the right to think of "them" as individual "things." If we say that causality "does not hold" in physics, we must add that it does not hold when applied to items which themselves we cannot as yet conceptualize. If we precipitously pronounce that causality is *passé*, we may be making an altogether unjustified translation of our abstract symbols on the basis of a mere analogy between these "unthinkable" particles and the things that we normally talk about.

WHY ARE SOME THINKERS SO REACTIONARY ABOUT DETERMINISM?

Psychiatrists have come to think that firmly held views, no matter how apparently bizzare and unreasonable, turn out on examination to have some kind of almost reasonable foundation. Psychiatrists, therefore, have less excuse than philosophers when they dismiss popular and tenacious doctrines as mere habit or ignorance. This applies to the notion of determinism. Anyone who feels that determinism is a naive belief should refer to the little-known work of Emile Meyerson who traces the impact of this belief on the progress of science up to his day. He offers a very persuasive argument that science (and explanation in general) is an attempt to find that what looks like a change is in fact the expression of something that does not change; that we seek an identity underlying an apparent diversity, and that the idea of causality is a part of the very process of explanation.

Be that as it may, it behoves the critic of this ancient notion to show in detail that he can do without it, and to date this has not been done. I am tempted to adduce Angel's apparent belief that a random distribution of neuronal elements might *cause* a psychological effect. But far more important are the difficulties encountered by indeterminist philosophers of science in coping with problems which are in fact the gaps left in their systems by the rejection of causality.

DETERMINISM WITHOUT CAUSALITY?

Russell's translation of determinism from causal terms into terms of functional relations is incomplete, and, by virtue of being incomplete, argues against the notion of causality by begging the question, and stores up trouble for him in another area. Someone who believes in causality believes that the description of a system at a given time is a function of certain data at other times, and this much Russell has accounted for in his formula. But a believer in causality also believes that the relationship holds even if the data are not observed, and the events they describe do not happen. In other words, causality implies that *if* the data are as described in the antecedent part of the formula, *then* the final description also

applies, even when the data are not so described ; in that case he says that a given state *would have* been observed, if the requisite data *had been* noted. This last phrase is known as a "counter-fact conditional" statement, meaning that it says something about what would have resulted if something which did not happen had happened. This part of the meaning of causality is not comprehended within Russell's formula, and it is omitted precisely because such a statement involves the notion of causality which it is Russell's purpose to dismiss. Now the impressive fact, the significance of which cannot be exaggerated, is that modern philosophy of science finds as great a need for counter-fact conditional statements as did ancient philosophy. To convince oneself of this, one need only note the profusion of attempted formulations of the counter-fact conditional. And it is really no surprise at all because a moment's reflection will reveal that if one cannot make counter-fact conditional statements, one cannot predict events in the future. Russell's formula is a case in point. His notation limits him to statements about relations between things that have actually happened. There is no room in it for invisible or latent tendencies which have never manifested themselves. But it is belief in these dormant "tendencies" (usually called laws of nature or causal chains) which leads us to predictions, because at the time when we want to make a prediction, the "tendency" we are counting on has not yet yielded up its manifestation. When the future arrives, we can describe the relation between conditions at two times according to Russell's formula, but while the future still lies before us, the hints about what it will be like can in no way be accommodated in Russell's formula. In short, there is no room in Russell's functional relationships for "tendencies" or "dispositions (to react in certain ways)," and without these we can make no statements about what does not yet exist. But science is nothing if not predictive.

Although visibly disturbed by it, modern philosophy of science has not resolved this dilemma, and its failure to do so leaves it open to the charge that the counter-fact conditional statement represents in modern philosophy of science what causality represents in the older systems, and the generally recognized necessity for such statements is an indication that modern science like ancient science employs the notion of causality.

IS THE QUESTION OF DETERMINISM AN EMPIRICAL ONE?

Finally, Angel's statement that the question of determinism "cannot be answered *a priori*" hinges on his accepting Russell's functional translation of the notion of determinism. We have seen that Russell was attempting to confine his translation to descriptions of what has actually happened (omitting what might have happened). Because he limited himself in this way, he made determinism an empirical hypothesis : he says to look and see if a relation can always be established between all the states of a system at all moments. Angel is correct in saying that we cannot answer this question *a priori*. But if we are to have the power to say what would have happened and what will probably happen, we may need to invoke a principle of causality, and this principle may well *have* to be *a priori*.

Behind the great sophistication of modern probability theory lurks one uneasy commonplace, namely that no mere empirical description of what is or has been, offers by itself a valid reason for expecting something in the future, no matter how tentatively and probabilistically we hedge our expectation. Hume and Kant may be of some use to us yet.

Philosophy is like a closed surgical glove partly filled with air. If one attempts to collapse one finger it will tear a hole elsewhere in the glove. The fingers represent concepts, for example causality. The history of philosophy is replete with schools that have nicely done away with problems or concepts that have concerned other schools, only to find that they are struggling just as hard with problems that are merely the manifestations of the same pressure that forced the others to their conclusions. Perhaps this is the most important lesson to be learned from the history of philosophy.

CONCLUSIONS

1. It is not yet clear what conclusions we may draw from the Indeterminacy Principle

of physics in regard to causality or determinism.

2. We must explain why many thinkers hang on to the concepts of causality and determinism by factors of sufficient weight to account for the tenacity with which they hold on.

3. Determinism, as it is commonly thought of, has not as yet been expressed in non-causal terms, and causality or its proxies still appear to be essential concepts in scientific thought.

4. Attempts to make the question of determinism an empirical one have not been successful.

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United States Naval Hospital,
Navy No. 3923,
F.P.O. San Francisco, Calif.

REPLY TO FOREGOING

Editor, THE AMERICAN JOURNAL OF PSYCHIATRY :

SIR : It was a pleasure to learn that my paper has been studied so carefully and answered so intelligently by Dr. Friedman. I would agree with him that one should "underline the tentativeness" of my conclusions. The purpose of my article was certainly not to provide a definitive statement on the current status of causality or to insist that psychiatry must keep in step with physics. Another point on which I would agree with Dr. Friedman is the danger of using—or abusing—the concepts of modern physics in fields where they do not apply. In this connection, I feel that one should be very cautious in applying Heisenberg's principle as Eccles has done. Although I referred to Eccles' use of the Indeterminacy Principle in neurophysiology, I cannot accept responsibility for the validity or invalidity of Eccles' argument. My object in paraphrasing Eccles was merely to show one possible mechanism whereby neuronal activity may be randomized, at least partially.

After my article was submitted for publication, Russell's new book, *My Philosophical Development,* was released. Dr. Friedman will be pleased to learn that Russell is now less definite in his rejection of causality. On p. 17 Russell writes, "Cause, which was the philosophical form of what physicists called force, has also become decrepit. I will not admit that it is dead, but it has nothing like the vigour of its earlier days." Moreover, in chapter XVI, Russell invokes the notion of "causal lines." So it looks as if the "old headaches" may be with us for some time. If my article had been written a few months later, some of the remarks about causality would have been even more tentative.

Entirely aside from the question of causality and determinism, I think that it is important for psychologists and psychiatrists to be aware of the growing interest in information theory and the importance of "random" factors in biological systems. The current literature on mathematical biophysics contains numerous references to "Markov chains" and "Markovian machines," in which behavior is determined by a matrix of transition probabilities. Here is an example which should be of great interest to psychologists : At the University of Illinois, high-speed digital computers are now being used to compose music, imitating, to some extent, the thought-processes of human composers. When the machine was set to composing melodies, it was programmed so as to generate random integers. Different degrees of randomness could be introduced in order to achieve a "compromise between chaos and monotony." The point that I wish to make is this : that even the computing machines are now being programmed so as to include random factors in their behavior, especially when they are supposed to be imitating "human" activities.

In my article I suggested that normal human behavior may involve some sort of balance or compromise between order and disorder, between randomness and rigid organization. I feel that this concept will assume more and more importance in future analyses of human behavior. This concept does not need to involve those "intractable headaches of philosophy" at all. We are faced with a very practical and important question : to what extent does the "programming" of the human brain include random factors ? Without allowing for randomization, we cannot understand the operation of ILLIAC, much less the human mind.

In summary, I feel that my statements

about causality should be modified in the light of Russell's recent statements on the subject. Nevertheless, I feel that psychiatrists should pay very serious attention to recent developments in information theory, systems analysis, and programming of computers, in which random factors are recognized as very important. We may discover that normal human behavior is a compromise between order and disorder, susceptible to abnormal deviations in one direction or the other.

Ronald W. Angel, M.D.,
VA Hospital,
Hines, Ill.

WISDOM

Here is the test of wisdom
Wisdom is not finally tested in schools,
Wisdom cannot be pass'd from one having it to another not having it,
Wisdom is of the soul, is not susceptible of proof, is its own proof.

—Walt Whitman

OPINION

Nothing is more curious than the self-satisfied dogmatism with which mankind at each period of its history cherishes the delusion of the finality of its existing modes of knowledge. Sceptics and believers are all alike. At this moment scientists and sceptics are the leading dogmatists. Advance in detail is admitted : fundamental novelty is barred. This dogmatic common sense is the death of philosophical adventure. The Universe is vast.

—Alfred North Whitehead

NEWS AND NOTES

Harvard Research Training Program.—This program is designed for selected psychiatric residents after their third year of training, or Ph.D.'s interested in mental health research careers. The candidate will be attached to one of 8 laboratories (clinical psychiatry, social science, psychology, psychophysiology, psychopharmacology, neurochemistry, and neurophysiology), will participate in interdisciplinary seminars, and will receive other special instruction. The training period may be for one or more years, to fit a man for a career in research or academic life. The stipend: $6,000 for the first and $7,000 for the second year of training, beginning in July of 1960. For information address the Research Department of the Massachusetts Mental Health Center, 74 Fenwood Road, Boston.

Dr. Barnes Woodhall Appointed Dean Duke Medical School.—Neurosurgeon Barnes Woodhall assumes the deanship of Duke University Medical School July 1, 1960, succeeding Dr. Wilburt C. Davison, Professor of Pediatrics in the Medical School. Dr. Woodhall, many years a member of the faculty of Duke Medical School, is V.A. consultant in neurology, treasurer to the Second International Congress of Neurological Surgery 1961, and member of the executive council of the World Federation of Neurosurgical Societies.

Dr. Bernard Wortis Dean N. Y. U. School of Medicine.—Succeeding Dr. Donal Sheehan, professor of anatomy, New York University has announced the appointment of Dr. S. Bernard Wortis as dean of the School of Medicine and Post-Graduate Medical School and deputy director of the N. Y. U. Medical Center. Dr. Wortis will also continue as professor of psychiatry and neurology in the School of Medicine.

British International Medical Advisory Bureau.—The Council of the British Medical Association has established this Bureau with a view to welcoming and providing a personal advisory service to physicians visiting the United Kingdom. Information is available at the Bureau on postgraduate education facilities and visits to hospitals and clinics. General information, advice as to lodging accommodations, *etc.*, will also be available. Those wishing to visit hospitals or seeking advice about postgraduate courses should provide the Bureau with advance information as to professional experience. All communications should be sent to: The Medical Director, International Medical Advisory Bureau, Tavistock Square, London, W.C. 1, England.

The American College of Nutrition.—Formation of the American College of Nutrition was announced on October 26 by a group of New York and New Jersey specialists in nutrition, metabolic diseases and gastroenterology. The college will include physicians, gerontologists, endocrinologists, surgeons and others. Its purpose is to promote postgraduate research and education in therapeutic nutrition.

The college is incorporated as a nonprofit organization subject to American Medical Association regulations. Its annual meeting will precede the AMA convention in the same city each year, with the first meeting scheduled for Miami, Fla., in 1960. The administrative office of the college: 19 Oak St., Livingston, N. J. Dr. S. William Kalb of Newark, N. J. has been elected president, and Dr. Robert A. Peterman of Livingston, N. J., secretary-treasurer.

The Chilian Society of Neurosurgery.—The Third annual meeting of the Society will be held at Antofagasta, July 22-24, 1960.

Subject: Neurological and Neurosurgical Sequelae of Birth Trauma.

President: Dr. Carlos Villavicencio.

Secretary-General: Dr. Juan Fierro M.

For information write to the Neurosurgical Society of Chile, Cassilla 70-D, Santiago.

COMMUNICATION AIDS IN APHASIA.—A one-hand manual language was developed by Dr. Hamilton Cameron of New York City as a result of his own disability from right hemiplegia and complete aphasia resulting from a cerebral embolism in 1943. Using his left hand he devised 20 hand signs which are pictured on a "Hand Talking Chart" that can be had without charge by doctors and nurses who may have use for it in their practices.

The International Research Council was chartered in 1954 as a world-wide medical organization for the collection and dissemination of knowledge concerning aphasia associated with hemiplegia.

Further information may be obtained from Dr. Cameron at 601 W. 110th St., New York 25, N. Y.

THE AMERICAN BOARD OF PSYCHIATRY AND NEUROLOGY, INC.—The Board will conduct its Spring, 1961, examinations in New Orleans, La., on March 20-21, 1961.

SEMINARS ON HYPNOSIS FOUNDATION.—A non-profit teaching and research institute has been incorporated and licensed by the State of Illinois. It is currently conducting courses in clinical hypnosis under the sponsorship of various medical and dental societies and universities, giving repeated courses of both beginning and advanced instruction to give full coverage over a two-year period.

President of the Board of Governors is Milton H. Erickson, M.D. Official headquarters : 6770 North Lincoln, Chicago 46, Ill. It is serviced by a teaching staff of more than 20 clinically experienced lecturers from the fields of medicine, dentistry and psychology.

INTERNATIONAL SYMPOSIUM, "THE EXTRAPYRAMIDAL SYSTEM AND NEUROLEPTICS."—The Department of Psychiatry of the University of Montreal is organizing this international symposium to be held at the University of Montreal, November 17-19, 1960. The purpose is to permit an exchange of ideas among researchers interested in this subject from the point of view of anatomy, physiology, neurosurgery and psychiatry. Admission to the symposium will be unrestricted but participation will be by invitation only. We have the cooperation of our 10 Canadian provinces, the United States and several European countries. The official languages are English and French, with simultaneous translation. For information, address to : Doctor Jean-Marc Bordeleau, Department of Psychiatry, University of Montreal, Montreal, Canada.

AMERICAN PSYCHOSOMATIC SOCIETY.—At the annual meeting of the American Psychosomatic Society in Montreal, March 25-27, 1960, the following persons took office : President, Morton F. Reiser, M.D. ; President-elect, Stewart Wolf, M.D. ; Secretary-Treasurer, Eugene Meyer, M.D.

Elected to Council positions were : John I. Lacey, Ph.D. ; John W. Mason, M.D. ; and John P. Spiegel, M.D.

The eighteenth annual meeting of the Society will be held on April 29 and 30, 1961, in Atlantic City.

SUMMER WORKSHOP AT VINELAND.—Sponsored jointly by Temple University and the 72-year-old Vineland Institution, the annual Summer Workshop for teachers and prospective teachers of retarded children will be held from June 27 to August 5, 1960.

The Training School at Vineland has a notable history in the field of education and training for the retarded. It was the first to establish a research centre in retardation ; it first standardized intelligence testing, and it developed early research in cerebral palsy and the Vineland Social Maturity Scale.

For information, contact The Director of Summer Sessions, Temple University, Philadelphia 22, Penna.

LYNCHBURG TRAINING SCHOOL LECTURES.—From May 26 to Oct. 26, 1960 The Lynchburg (Va.) Training School and Hospital will conduct a series of 16 lectures, seminars, and demonstrations in the fields of mental retardation, learning process, special education, neurological diseases of childhood, electroencephalography, schizophrenia, and psychotherapy.

Distinguished speakers will come from Letchworth Village, Medical College of

Virginia, Columbia University, Boston and University of Virginia.

General Semantics General Conference.—The Conference will be held in Hawaii, July 31-Aug. 4, 1960. A package ticket at $349.75 includes round trip by air, registration for the Conference, 7 days in Hawaii with 6 nights at the Hawaiian Village Hotel, special tours and meals.

Arrangements for attending the Conference should be made at once through Andrew W. Lerias, Inc., 133 Montgomery St., San Francisco.

Eastern Psychiatric Research Association World Tour.—Members of the Association will start this tour July 1, 1960. They will hold meetings in conjunction with the psychiatric societies of Japan, Thailand, Hongkong, India and Israel. At these meetings members of the Association will present papers.

Anyone who would like to join the tour should contact Mr. C. J. Jones, University Travel Co., 18 Brattle St., Cambridge, Mass.

An all-inclusive round-trip fare will be approximately $2,200. to $2,300. per person.

The American Board of Psychiatry and Neurology, Inc.—The following candidates were certified by this Board after examination in San Francisco, Calif., March 14 and 15, 1960.

PSYCHIATRY

Ackerman, Norman Mactas, Woodmere, N. Y.
Amselem, Benmaman Jaime, Agnew, Calif.
Arlen, Harold W., Beverly Hills, Calif.
Bail, Bernard W., Beverly Hills, Calif.
Ball, Thomas Frederick, San Carlos, Calif.
Belcher, Charles H., Winnebago, Wisc.
Berenson, Marvin Harvey, Beverly Hills, Calif.
Binstock, William A., Topeka, Kan.
Blanchette, James E., Redlands, Calif.
Boyce, William H., Philadelphia, Pa.
Braun, Joseph A., Topeka, Kan.
Braun, Robert A., Detroit, Mich.
Breiner, Sander James, Detroit, Mich.
Briggs, Leon Royden, Jr., Fresno, Calif.
Brown, George Clark, Oakland, Calif.
Cambor, Charles Glenn, Mayview, Pa.
Cheatham, James S., Seattle, Wash.
Chen, Calvin H., Northville, Mich.
Cooper, Arnold M., New York, N. Y.
Errichetti, Anthony Joseph, Jr., San Francisco, Calif.
Eshleman, S. Kendrick, III, Lancaster, Pa.
Fischer, Ames, San Mateo, Calif.
Follette, William T., San Francisco, Calif.
Freeman, Paul, San Francisco, Calif.
Furukawa, Edward F., Philadelphia, Pa.
Gerz, Hans Otto, Middletown, Conn.
Graham, Charles R., Berkeley, Calif.
Granzow, O. Joachim, Los Angeles, Calif.
Griffin, Julius, Encino, Calif.
Grotstein, James Stanleigh, Beverly Hills, Calif.
Guido, John A., Fullerton, Calif.
Haberle, Charles A., Minneapolis, Minn.
Haentzschel, Lester E., Salem, Ore.
Haylett, Clarice H., San Francisco, Calif.
Hermann, Harland T., Fort Meade, S. D.
Hernandez, Manuel O., Worcester, Mass.
Hoyer, Thomas V., Van Nuys, Calif.
Ionedes, Nicholas, Hines, Ill.
Johnston, Harold B., Tacoma, Wash.
Kane, Ruth Powell, Pittsburgh, Pa.
Karn, William Nicholas, Jr., Evanston, Wyo.
Kaupas, Julius V., Eloise, Mich.
Kenward, John F., Chicago, Ill.
Kinsman, Robert G., Fullerton, Calif.
Kline, Frank M., Beverly Hills, Calif.
Knight, James A., Houston, Tex.
Koegler, Ronald R., Los Angeles, Calif.
Kramer, Charles H., Oak Park, Ill.
Langsley, Donald G., San Francisco, Calif.
Lanning, Theodore R., Brooklyn, N. Y.
Larson, Alfred Leonard, San Rafael, Calif.
Lebovitz, Allen E., Pittsburgh, Pa.
Leone, William A., Miami, Fla.
Lightburn, John L., Denver, Col.
Linton, Patrick H., Topeka, Kan.
Marshall, John D., Jr., Westport, Conn.
Marty, Samuel C., Jr., San Mateo, Calif.
Mason, Edward, Worcester, Mass.
Mawardi, Youssef K., Los Angeles, Calif.
Mayberg, Donald MacMillan, Minneapolis, Minn.
Mercer, Wayne C., San Francisco, Calif.
Merjanian, Antipas, Agnew, Calif.
Morgenstern, H. S., Napa, Calif.
Neal, Miron W., Belvedere, Calif.
Nobel, Rudolf E., Lansing, Mich.
Orgun, Ibrahim Necmi, Hartford, Conn.
Osinoff, Maurice, New York, N. Y.
Paredes, Alfonso, Oklahoma City, Okla.
Parlour, Richard R., Beverly Hills, Calif.
Patterson, Robert M., Imola, Calif.
Pipe, Bernard Joseph, Tacoma, Wash.
Powell, Charles W., Cherokee, Iowa
Raulinaitis, Valerija B., Downey, Ill.
Rondeau, Henry Thomas, Arcadia, Calif.
Ross, Melvin B., Cleveland 6, Ohio
Schapire, Hans Martin, Denver, Col.
Shipper, John C., Los Angeles, Calif.
Sidley, Nathan T., West Newton, Mass.
Simmons, James Q., III, San Fernando, Calif.
Simson, Clyde B., Detroit, Mich.
Smith, Philip B., Topeka, Kan.
Spira, Henry, Birmingham, Ala.
Stamatovich, Constantine, Flushing, N. Y.
Tapia, Fernando, Clayton, Mo.
Traill, Alexander C., Denver, Col.
Turcotte, Guy N., Portland, Maine

Turner, David Allen, New York, N. Y.
Vaughn, Rufus M., Boston, Mass.
Voegele, George Edward, Columbus, Ohio
Washburn, Stephen Louis, Belmont, Mass.
Wasserman, Edward, Chicago, Ill.
Weckstein, Marvin S., Detroit, Mich.
Wells, Carl D., Berkeley, Calif.
Wertz, William John, Detroit, Mich.
Wheat, William Douglas, Baltimore, Md.
Winkler, Ralph K., Stockton, Calif.
Zemsky, Boris, Tucson, Ariz.

*Denotes Supplementary Certification

NEUROLOGY

Ajax, Ernest Theodore, Salt Lake City, Utah
Davis, Edward H., New York, N. Y.
Homer, Frederick A., Denver, Col.
Mauceri, Jennie, Woodside, N. Y.
Reinert, John E., Rapid City, S. D.
Thompson, Hartwell G., Jr., Madison, Wisc.
Tourtellotte, Wallace William, Ann Arbor, Mich.
*Walter, Richard D., Los Angeles, Calif.
Watson, James MacDonald, Syracuse, N. Y.
Webster, David D., Minneapolis, Minn.
Wells, Charles Edmon, New York, N. Y.

TIME AND ETERNITY

The now that flows away makes time, the now that stands still makes eternity.

—Boethius (480?-524)

MESOPOTAMIAN MEDICINE

A system of medicine that was dominated by magic and religion, and the purpose of which was to rehabilitate an individual and to reconcile him with the transcendental world, obviously included psychotherapy, the soul-searching of a patient who was convinced that he suffered because he had sinned had a liberating effect; and the rites performed and the words spoken by the incantation priest had a profound suggestive power. Mesopotamian medicine was psychosomatic in all its aspects.

—Henry E. Sigerist
A History ofMedicine

BOOK REVIEWS

J. M. Charcot, His Life—His Work. By *Georges Guillain, M.D.* Edited and translated by *Pearce Bailey, Ph.D., M.D.* (New York: Paul B. Hoeber, Inc., Harper and Brothers. pp. xvi + 202 incl. index. illus., 1959. $7.00.)

Charcot was one of those men who, in the words of Pearce Bailey, "cross the narrow boundaries between nations and belong to the whole world." He died in 1893. In 1955 was published the original biography in French by Professor Georges Guillain, who, too young to have been a pupil of Charcot, was however a pupil of several of the closest disciples of the Master, including Raymond, his successor, and Pierre Marie. Eventually Guillain occupied the same professorial chair at the Faculty of Medicine in Paris that had been created for Charcot.

The story of Charcot is intimately bound up with that of the great hospital of the Salpêtrière, "that grand asylum of human misery" as he called it, and which dates from the 16th century. The name derives from an arsenal where gunpowder was stored and which originally occupied the same site. Conversion to asylum and hospital uses with much new construction took place during the 17th century. The history of this famous institution is included in Guillain's text.

Charcot was born in Paris in 1825. He died at the age of 68. He had begun his studies in neurology in 1850; he came to the Salpêtrière in 1862, was appointed clinical professor of diseases of the nervous system in 1881, thus becoming the world's first professor of clinical neurology. In Bailey's words he "transformed the Salpêtrière from a prison and an asylum of unwanted womanhood into one of the great clinical research centers in the world."

Guillain in his dramatic story calls Charcot "the veritable creator of modern neurology." And he documents this statement with a list of some of the discoveries reported year by year from the Salpêtrière clinic; among them, intermittent claudication (1858); he was the first in France to describe exophthalmic goitre (1853); amyotrophic lateral sclerosis (1865), sometimes spoken of as "Charcot's disease"; amyotrophy Charcot-Marie, also described by H. H. Tooth of London the same year (1886); multiple sclerosis, named by Charcot "*sclerose en plaques*," and differentiated from paralyis *agitans* (1868); tabetic arthropathies (1868); spastic spinal paralysis, called by Charcot "*tabes dorsal spasmodique*" (1876). Erb had also observed this syndrome in 1875; cerebral localization studies (1870-1880); spinal cord localization (1873); hysteria and the neuroses (1862-1892).

Guillain has something to say about later criticisms of Charcot's description of hysteria "by some of the younger generations who have never read the works of Charcot." These remarks are timely because of the perennial tendency of certain writers of any period to misunderstand and even misrepresent the teachings of their predecessors with whose works they are not familiar.

Pierre Marie pointed out that originally in the service assigned to Charcot at the Salpêtrière epileptics and hysterical patients were indiscriminately housed. "Living in this way among the epileptics," wrote Marie, "the young hysterics were susceptible to powerful impressions and because of their tendency to mimic . . . they duplicated in their hysterical fits every phase of a genuine epileptic seizure." Guillain agrees that Charcot's account of the "major hysteric crisis with its four well-defined phases was unquestionably quite artificial and colored by acting on the part of some patients"; but he also lists the multiform symptoms recognized as characteristic of the hysteric neurosis and of which Charcot gave full descriptions. To the charge that he overlooked the problem of malingering his own words give the refutation: "It is found in every phase of hysteria and one is surprised at times to admire the ruse, the sagacity, and the unyielding tenacity that especially the women, who are under the influence of a severe neurosis, display in order to deceive . . . especially when the victim of the deceit happens to be a physician." Charcot also gave a full account of the condition later elaborated by Duprè as "mythomania."

Supplemental to the chapter on Charcot's hysteria studies Guillain adds a discussion of Babinski's theories.

Professor Leyden of Berlin spoke of the Salpêtrière of Charcot as "the center of the grand international march of neuropathology, where everything was to be seen and to be learned, and where almost every day something new was brought to light."

And Babinski in 1925: "To take from neurology all the discoveries made by Charcot would be to render it unrecognizable. Indeed, not a single day passes in a neurologic service

that we do not use some of the notions he introduced ; his thinking is always with us."

After his death, Charcot's pupils collected funds for making and erecting at the entrance of the Salpêtrière a bronze statue of their beloved Master. In 1942 Nazi troops occupying Paris caused this statue to be melted into scrap metal. Recording this bit of recent history Guillain said : "Here, I should like to abstain from comment."

Pearce Bailey has rendered an important service in making Guillain's book available to English readers. He has done much more than translating the French text ; he has added footnotes throughout explaining difficult French expressions and allusions to personalities and incidents that require familiarity with the French scene and French history. His version thus becomes easy and delightful reading.

A number of illustrations, including one of the bronze statue that is no more, accompany the text.

C.B.F.

Psychotropic Drugs. By *S. Garattini* and *V. Ghetti.*

This book, edited in Milan, Italy, contains the proceedings of the International Symposium on Psychotropic Drugs which was held in Milan in May 1957. This is the first major international symposium on psychopharmacological agents to appear since the advent of chlorpromazine and reserpine in the early 1950's. This volume of 606 pages contains 100 papers and short communications on a wide variety of drugs by authors from many countries. Both academic research reports and papers emanating from the laboratories of pharmaceutical companies are included. Three-fourths of the volume is devoted to basic research on brain function and on the behavioral electrophysiological and general pharmacological studies of psychotropic drugs. The basic work reported includes interesting papers by Miller, Killam, Olds, Bradley, Unna, Himwich, Norton, Blough, Morruzzi, Blaschko, and Garattini, to name just a few. The book is particularly valuable for those interested in the mechanisms by which drugs act on the central nervous system, the clinical work reported being, generally, more informal and giving more limited coverage of this aspect of psychopharmacology. Many of the contributions from France, Italy, and other European countries are printed in the language of the author. English summaries are provided for the major papers but not for the brief communications also included in this volume.

Jonathan O. Cole, M.D.,
Psychopharmacology Service Center,
National Institute of Mental Health.

Water and Electrolyte Metabolism in Relation to Age and Sex. Ciba Foundation Colloquia on Ageing, Vol. 4. Edited by *G. E. W. Wolstenholme* and *M. O'Connor.* (Boston : Little, Brown and Co., 1958, pp. 327. $8.50.)

This is the collection of papers and discussions about them presented at a colloquia held in January, 1958, in London under the auspices of the Ciba Foundation. The participants were 27 authorities in the field of water and electrolyte metabolism, from Europe and the United States.

Though many topics are covered, there is but little dealing specifically with the central nervous system and hence of direct interest to the neurologist or psychiatrist. The paper of Fourman and Leeson on hyponatremia and hypernatremia associated with cerebral disturbances was a critical review of this topic and the presentation of some case data ; their conclusions are that problems of hypernatremia are in general ones of water deficiency while those of hyponatremia are more complicated and can be associated with either simple salt depletion and/or excess dilution of extracellular fluid by water. Because of the recent interest in a magnesium deficiency syndrome, the paper of Card and Marks is also of some interest to neurologists and psychiatrists ; these workers emphasize the great difficulty there is in producing a sufficient depletion of body magnesium to cause clinical signs.

This book is not meant to be either a textbook nor a general review of the field ; the general tenor of the material presented and the discussions that follow the various papers is on the level of the expert in the field. The book will no doubt be of interest to those who already have a good background in this complicated field.

W. J. Friedlander, M.D.,
Boston, Mass.

Childbearing Before and After Thirty-five. By *Adrien Bleyer.* (New York : Vantage Press, 1958, pp. 119, $2.95.)

This unobtrusively published book is of great importance. It is by a remarkable physician who was for many years associate professor of clinical pediatrics at the Washington Uni-

versity School of Medicine. In 1906 he founded the first Infant Welfare Clinic in America. For many years he has been our *best* authority on mongolism, and in 1934, in an astonishingly brilliant piece of informed deduction, he correctly suggested the cause of mongolism in a paper entitled "Indications that Mongoloid Imbecility is a Gametic Mutation of Degressive Type," *Am. J. Dis. Child.*, **47** : 342-348, 1934. The explanation offered in this astonishing paper, as far as I know, was completely ignored, and it was not until January 1959 with the publication of Lejeune and his co-workers' paper, and in later months in 1959, that mongolism was demonstrated to be due to a disjunctive chromosomal aberration. The present volume was published too early to incorporate these new discoveries, and it was a reference to his 1934 paper on mongolism that led me to the discovery for myself of Dr. Bleyer's truly remarkable paper—which I should rank as one of the outstanding pieces of ratiocination in the whole history of science. It is really quite astonishing that it had to wait a quarter of a century for confirmation by workers who, I am sure, had never heard of Dr. Bleyer's paper.

In the present volume Dr. Bleyer resumes some of the evidence which demonstrates that the optimum age for childbearing is between about 21 and 28 years, and that as the mother's age advances every aspect of pregnancy, from the development of the embryo to labor and birth, is increasingly seriously affected.

Since the reviewer has been collecting material on this subject for many years, he can confirm the general validity of this demonstration. Readers of this Journal will be interested in the evidence bearing on the relation between age of parents, and particularly of mother at conception of the patient. There is no longer any doubt that in a significant proportion of cases there is not only a social but also a biological factor, in some way related to age of mother at conception of the patient, which plays some role in affecting the psychological development of the individual.

The social and biological implications of maternity after age 35 are of the most serious nature, and society, already much indebted to Dr. Bleyer, and the physician and social worker, will be grateful to the author for bringing together so much of the relevant evidence relating to an area of social and medical practice which is still virtually wholly ignored. Dr. Bleyer's book is short, readable, and convincing, so that even he that runs may read. Its size is in inverse proportion to its importance.

ASHLEY MONTAGU, PH.D.,
Princeton, N. J.

DIAGNOSTICA PSICHIATRICA. Parte Speciale. By *Agostino Rubino Idelson.* (Naples : Di E. Gnocchi E F., 1958. pp. 443. L.5.500.)

This is the second of a series of 3 volumes written by Rubino on psychiatric diagnosis. It is difficult to review a book devoted to this subject at a time when psychiatric diagnosis has lost importance in relation to the psychodynamic and therapeutic approaches. The various entities are here described as collections of symptoms, without any attempt being made to interpret or to connect them together. Perhaps the author will do so in the third volume.

Rubino is a pupil of Buscaino, who is one of the staunchest supporters in Europe of the organic school of psychiatry, believing that most psychiatric disorders are the results of metabolic dysfunctions. The whole book is written in the spirit of Kraepelin's and Buscaino's teachings. Bleuler has left very little imprint on the author, and such contributors as Adolf Meyer, Freud, Jung, Sullivan, Vigostki, Goldstein and others are almost completely ignored.

The book cannot be recommended as a complete textbook of psychiatry. However, certain sections, which are generally neglected in American psychiatric books, namely those dealing with rare organic syndromes like some types of mental deficiences, will be read with profit in this book by readers who are particularly interested in these conditions.

The book is richly illustrated.

SILVANO ARIETI, M.D.,
New York, N. Y.

THE EVOLUTION OF MAN'S CAPACITY FOR CULTURE. Edited by *J. N. Spuhler.* (Detroit : Wayne State University Press, 1959, pp. 79. $3.50.)

This is a most stimulating book containing 7 contributions to the fascinating problem how man evolved his capacity for culture. Dr. J. N. Spuhler considers "Somatic Paths to Culture" in a brilliant article, Ralph W. Gerard discusses "Brains and Behavior," S. L. Washburn speculates ". . . on the Interrelations of the History of Tools and Biological Evolution," Charles F. Hockett, makes a most original contribution in discussing the criteria of "Ani-

mal 'Languages' and Human Language," Harry F. Harlow, has some interesting views on "Basic Social Capacity of Primates," and in a paper of quite fundamental importance Marshall D. Sahlins discusses "The Social Life of Monkeys, Apes and Primitive Man." In a final "Summary Review" Leslie A. White ably discusses the issues.

ASHLEY MONTAGU, PH.D.,
Princeton, N. J.

CURRENT CONCEPTS OF POSITIVE MENTAL HEALTH. By *Marie Jahoda, Ph.D.* (New York: Basic Books, Inc., 1958, pp. 136. $2.75.)

Considerable interest will attend the reports of the Joint Commission on Mental Illness and Health. This Commission was established by the Mental Health Study Act of Congress in 1955. Eleven reports are expected during the next few months, of which this present monograph is the first.

In it, Dr. Jahoda reviews many of the more important attempts which psychiatric and social scientists have made to arrive at a satisfactory definition of good mental health. The theoretical assumptions and the evidence pro and con are briefly but clearly described. In approaching this very difficult task the author seeks the answer to such questions as: What is the essence, the core characteristic of positive mental health? How does it manifest itself? How can it be appraised and measured? What influences it for better or worse and how can it be protected or enhanced? As might be expected, the amount of theoretical material available is tremendous but the nature and extent of supporting scientific evidence is small.

Inevitably, value judgments must be made. We have to decide that one quality (or behaviour characteristic) is desirable, while another is not. And the decision, obviously, can only be made in a context of moral and ethical preferences, in addition to cultural and social considerations. The author concludes, a little sadly, that her review "does not resolve the complex problem of clarifying the psychological meaning of Positive Mental Health." Certainly the evidence indicates that there is no one kind of good mental health, any more than there is one kind of mental illness. Similarly the environmental and biologic conditions making for good mental health are difficult to isolate and study. "Those dissatisfied with this unending search for better and better approximations to an unattainable goal will have to turn away from science and seek elsewhere for their insight into conditions for mental health."

In spite of these discouraging conclusions Dr. Jahoda has done an extremely competent job of evaluating critically the current thinking in this area. She establishes 6 basic concepts relating to good Mental Health for which there is considerable support among scientific workers. These are: 1. Attitudes toward the self; 2. Growth Development and Self Actualization; 3. Integration; 4. Autonomy; 5. Perception of Reality; and 6. Environmental Mastery. The empirical basis for her selection, the conditions required for their observation and the specific nature and design of further research necessary are indicated.

The author's base line in this monograph is the premise that positive or "good" mental health is essentially different from the mere absence of mental illness and that no dichotomy exists separating on a continuum the one from the other. In this assumption she is in good company, for it is one entirely consistent with the definition of health contained in the charter of the World Health Organization. And this has been officially subscribed to by some 70 different countries!

However, it is *not* subscribed to by all psychiatrists, as is attested by Dr. Walter Barton in the concluding chapter (The Viewpoint of a Clinician). Many physicians will feel more at home with his contention that concepts involving psychobiologic and physiologic equilibrium (*e.g.*, Cannon's Homeostasis) are useful and that perhaps after all from a practical point of view, when a patient recovers from his mental illness and loses his mental symptoms he returns to a state of "good" mental health.

J. D. GRIFFIN, M.D.,
General Director,
Canadian Mental Health Association.

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2. Perform regular liver function tests.
3. In all instances daily dose should not exceed 12 mg.
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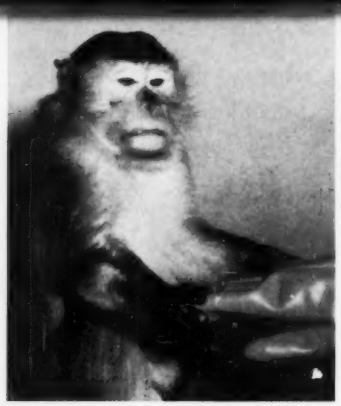

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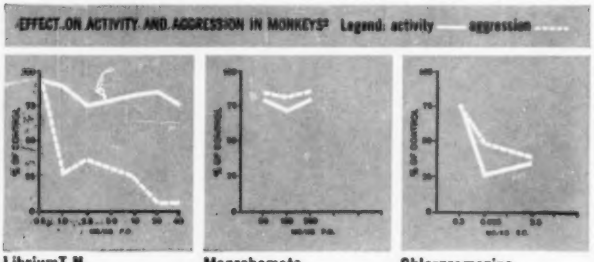
Calming and taming effect of Librium on rats, monkeys and mice



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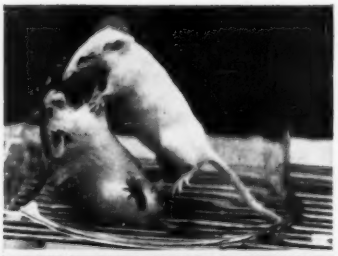

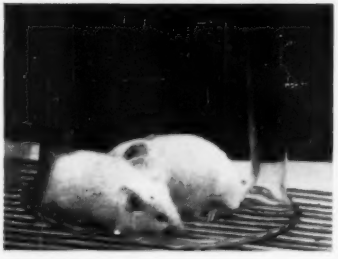

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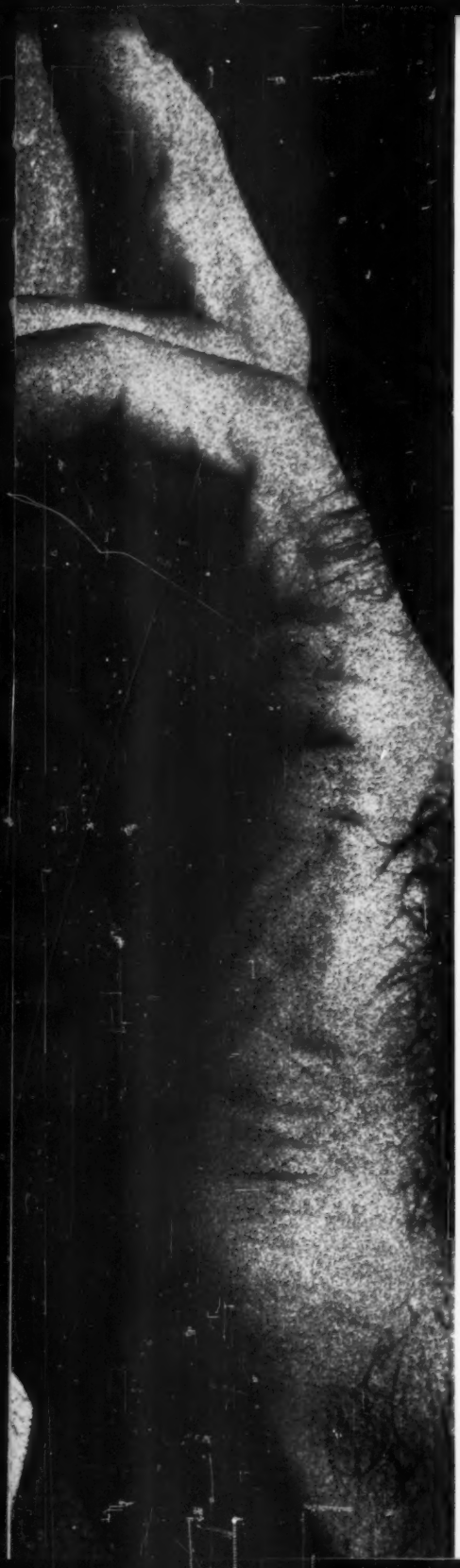

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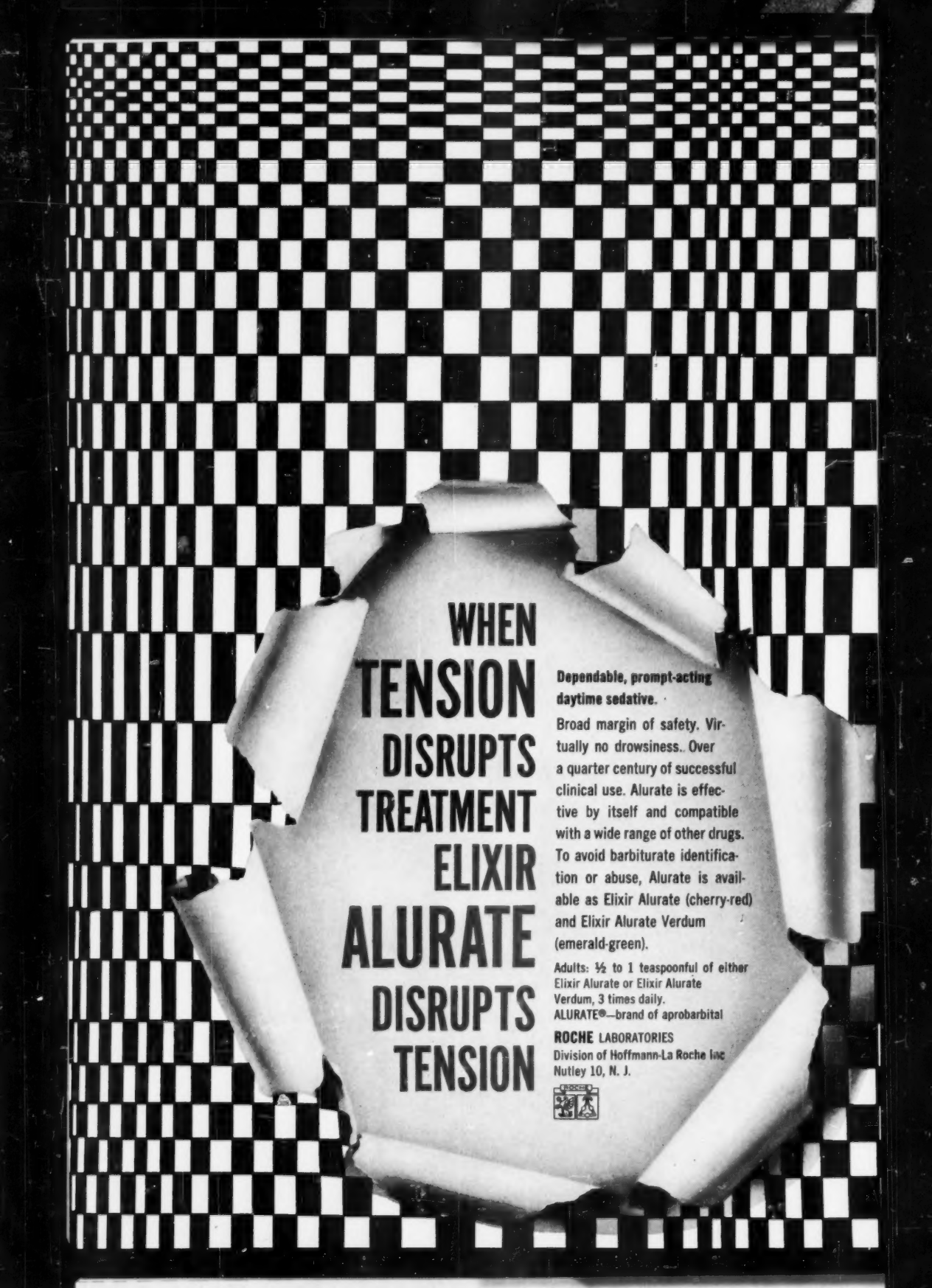
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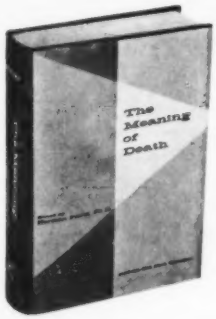

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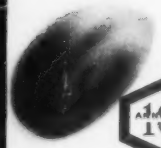

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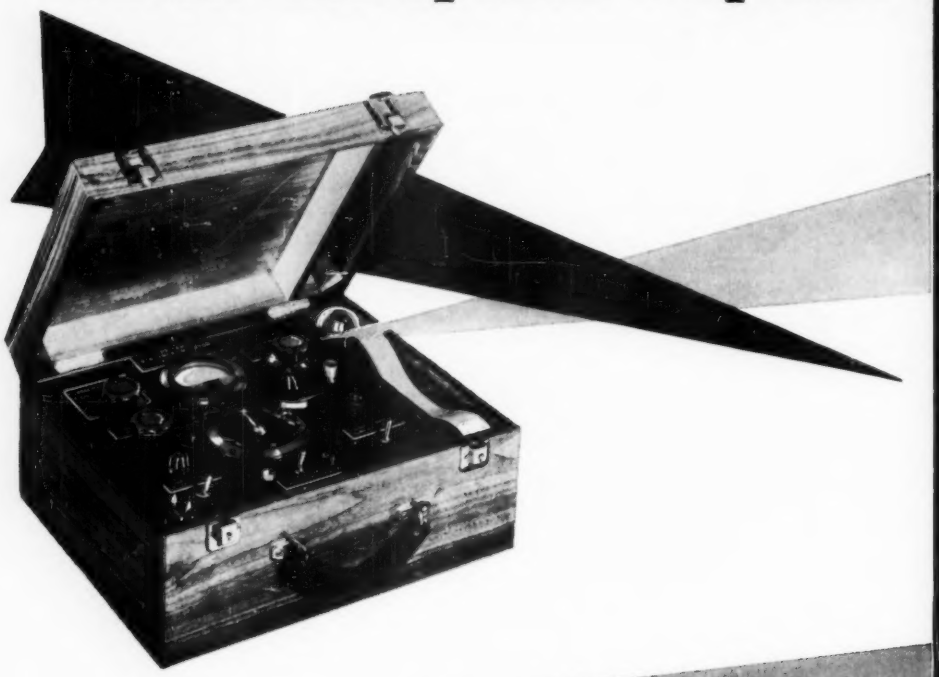

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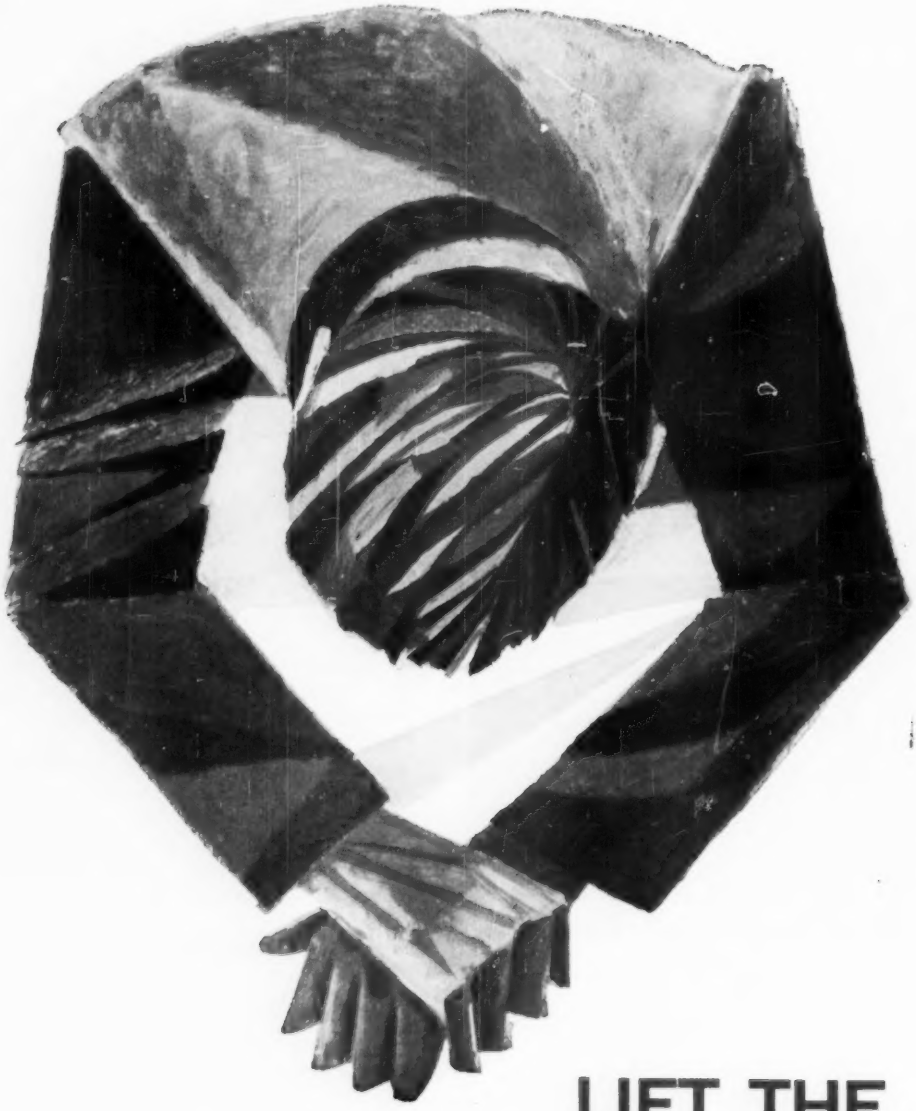

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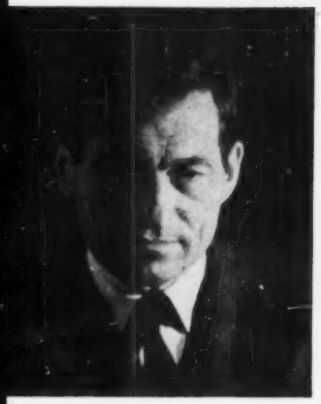

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